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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1862, *TO JUNE* 1863.

VOL. XXIII.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS
OF THE ROYAL ASTRONOMICAL SOCIETY.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXIII. *November* 14, 1862. No. 1.

Dr. Lee, President, in the Chair.

Lieut. Bailey, R.N., H.M.S. Excellent;
Lieut. H. Morland, Ebury Street, Pimlico; and
David Hornby, Esq., 46 Guildford Street,
were balloted for and duly elected Fellows of the Society.

Letter from Prof. Grant to the Editor.

I send you herewith, for insertion in the *Monthly Notices*, a communication which I have received from Prof. Smyth, containing a description of the great equatoreally mounted Refractor, of 11 inches aperture, erected a few years since by J. W. Grant, Esq., of Elchies, Morayshire; also some results of observations made by Prof. Smyth with the same instrument, on the occasion of a visit which he recently paid to Mr. Grant. It may be mentioned that the owner of this magnificent instrument resided many years in India, during which he employed much of the leisure time of an official life in cultivating scientific studies of various kinds, occupying himself occasionally with a 5-foot refractor of excellent quality. As an instance of what an observant eye may effect under favourable circumstances of climate, even although aided by an instrument of only inferior power, it may be stated that the duplicity of *Antares* was recognised by Mr. Grant as early as the 23d of July, 1844, that is to say, *two years anterior* to the discovery of the same interesting fact by the American astronomer, Prof. Mitchell, with the aid of the great Cincinnati Refractor (*Monthly Notices*, vol. xvi. p. 56). Prof. Smyth was recently led to a knowledge of this circumstance by an exami-

nation of Mr. Grant's observation papers. That it was not communicated earlier to the world is accounted for by Mr. Grant's retiring disposition, and by the modest estimation which he has invariably formed of his scientific labours. Prof. Smyth purposes communicating to the Society, on an early occasion, an account of Mr. Grant's astronomical observations in India.

Upon his return to Scotland, about twelve years ago, with the intention of spending the remainder of his life on his patrimonial estate, Mr. Grant resolved to continue his astronomical observations; and with a view to this object, he erected at Elchies* a substantial Observatory, which he fitted up with the great Equatoreal of 11 inches aperture, to which I have already referred. This was the first equatoreally mounted Refractor of considerable magnitude which had hitherto been erected in Scotland: indeed, with the exception of the Equatoreal of nine inches aperture subsequently acquired by the late Sir William Keith Murray, it is the only instrument of the kind deserving of the name which has ever been mounted in the northern part of this island. The unsuitableness of the climate of Elchies for astronomical observations, and recently continued ill-health, have prevented Mr. Grant from availing himself so fully of the advantages of this noble instrument as his Indian experience entitled him to hope for, and as the munificent sacrifice made by him in the cause of science so well merited.

At Prof. Smyth's request I have appended one or two footnotes to his communication, as, from the circumstance of his present temporary residence in the country, his access to original authorities has been necessarily limited.

Observatory, Glasgow, October 13th, 1862.

Experiences with the Elchies Equatoreally mounted Refractor of 11 inches aperture, in September 1862. By Prof. C. Piazzi Smyth, Astronomer Royal for Scotland.

Description of the Instrument.

This is a very remarkable instrument, as well in its mechanical as its optical portions, and I have had an unusual opportunity of becoming practically acquainted with both, during a continued stay of three weeks; for the whole of which time the key of the Observatory, and complete control over,

* Prof. Grant informs me that Elchies is situate about eight miles below the junction of the Spey and the Avon.—Ed.

and employment of, everything therein, were kindly conceded to me by the owner, and with the frequently repeated instructions that I was to employ them as freely as if the entire Observatory had been my own property.

From long remaining unused, a good deal of dust and thickened oil had to be removed before beginning work; but, that once done, all the essentials of the instrument were found in excellent condition, and micrometer observations of double stars were from that time made every fine night, and with a great feeling of satisfaction in the use and working of the instrument.

Optical Parts.

The object-glass is 11 inches in diameter and 16 feet in focal length. It has a number of minute bubbles in it (as indeed almost every object-glass has, and without further detriment than merely the loss of as much light as they intercept); but I have not been able to see any trace of striæ or positive imperfections in the glass, either by looking at or through it; or, by examining the discs of stellar light, by viewing a bright star, *α Lyræ*, much out of focus, and with a magnifying power of 397.

This power has been employed in all the observations, and with it, notwithstanding the very untoward state of the weather, some excellent proofs have been obtained of both the light-transmitting and the defining powers of the object-glass; though it is believed that better proofs still might be obtained, if a fully good night were to occur, and more difficult test objects to be above the horizon. The results, however, as far as yet secured, may be stated thus:—

In the course of examining a list of twenty-five well-known double stars, there were six cases where one or more additional small stars, not previously known or recorded, were discovered; besides two or three cases where small stars, whose existence had merely been mentioned by other observers, were found bright enough to admit of micrometer measurement in the Elchies telescope, with a position and distance wire micrometer and lamp-illuminated field.

These instances, of which a special list will be appended, and which speak chiefly for the light-transmitting power of the object-glass, were attained, even in spite of such bad definition of the atmosphere, that every star was more or less blurred and nebulous. There have indeed been only two occasions, and then only for a few minutes each time, when the atmosphere quieted and allowed the defining power of the object-glass itself to be studied; the first of these instances was on Sept. 17th, at 1^h P.M., when the Pole star was in the field, and looked a minute and brilliant disk with well-shaped thin rings around it, in the bright sunshine; the second was on Sept. 20th, at

5^h 30^m A.M., when B of γ *Andromedæ* was distinctly seen two different and completely separated stars, which w measured accordingly both in position and distance. Wl that it was a triumph to the object-glass to see them in t manner, I may mention that the disk of each star was c siderably less than the diameter of the fine spider's thread the micrometer, and about 0·3, or 0·4 of a second of space diameter; the distance apart, from centre to centre, being ab 0·6 of a second.

In completion of the optical details it may be mention that there are twenty-three eye-pieces of various powers, addition to the two of the micrometer; twenty-four caps w coloured glass, several piles of coloured glass disks, screw into each other, a fine totally reflecting prism of very large s a dynameter, &c., in two mahogany cases; besides the posit wire micrometer in its own box. The telescope has th finders, one of them being a fine 5-foot telescope, and is furnis with an illuminating apparatus for use with the micrometer

Mechanical Parts.

The Equatoreal Motion.

The mounting of the Elchies telescope is on the princ known as the German, in contradistinction to the English, the form with a long polar axis, and the telescope in the mic between the bearings.

The German form, viz., with short polar axis and telesc outside both bearings, is perhaps more frequently employed every country than the other, and specimens of it in connex with large telescopes are now numerous; but nowhere over whole world has so firm, and solid, and well-constructed example ever been mounted as that of Elchies.

This is saying a great deal, especially in knowledge of admirable models sent forth to various public and private servatories by the astronomical factories of Munich, Paris, York; but I believe it to be perfectly true, and owing, grea to the late Mr. A. Ross, the optician of the telescope, hav secured the services of Messrs. Ransome and May, Engine Ipswich, for the mounting. Messrs. Ransome and May w the firm who had been already selected and employed by Astronomer Royal, to introduce into the new Greenwich ins ments the solidity and improved methods of modern engineeri and I know that they entered with great zeal into the effic carrying out of Mr. Ross's order, Mr. May himself person assuring me, that he believed this instrument would be beginning of a new era in the construction of Equatoreals, reason of the astonishing firmness and steadiness, combi with truth, which he expected the mass of metal introdu into this one, and duly turned and planed, would impart to

movements. It was this mounting, accordingly, which attracted such general attention and commendation in the centre of the nave of the British portion of the International Exhibition in 1851, and it commends itself equally now, after eleven years standing in an Observatory.

The declination and polar axes, each about 6 inches in diameter, in solid metal, and 5 feet long, are wonders of strength in connexion with any optical instrument, and could never have been produced by any mere optician; while the gun-metal bearings, and gun-metal circles, together with the gearing and ungearing apparatus for the endless screws, is of a majesty of form, size, and substance, more like steam-engine, than ordinary telescope, fittings.

Both circles, 30 inches in diameter, are cast as disks of gun metal, without any perforations, about 1 inch thick, additionally strengthened towards the centreing, and furnished with rack edge and noble endless screws to work in them, of actually 2·1 inches in diameter; while their end-shake and wear in their bearings is corrected by the efficient method adopted in the shaft of a screw steamer; or, by having a long series of projecting flanges cut on the pivot, and working into a similar series of depressions in the gun-metal bearing.

It will thus be seen that this part of the entire instrument is quite worthy both of the engineering fame of Messrs. Ransome and May, and of their training for astronomical structures under George Biddell Airy; and to their work Mr. Ross has added various fine fittings, graduation on the circles very easily read, an excellent system of double Hooke's-joint handles, workable from the eye-piece of the telescope, and an efficient equatoreal driving clock.

The Stand.

The mere stand or pier for supporting the bearings of the above-mentioned equatoreal axes of motion, is also a noteworthy point about the Elchies instrument. It is of iron, and by Messrs. Ransome and May also; there are the usual adjustments for the bearings of the axes, and the general form is convenient; but the remarkable and unprecedented thing is the extraordinary amount of weight introduced into it, for the purpose of checking those minute tremors which usually disturb accurate vision in all of the ordinary forms of mountings of large telescopes; and which can only be thoroughly corrected by the absorbing effect of mass. One portion of the stand accordingly weighs no less than eleven tons, and is the greatest quantity of iron ever cast into a single piece for any astronomical instrument of any age or in any country.

In conclusion I have to add, that though the telescope be so large, and the mounting so heavy, and though I have never had experience in the use of so large a telescope before, yet I found

no difficulty in managing this one without any assistance, could point from star to star by their places on the circles, with as much rapidity as on many much smaller instruments; could bisect the stars on the micrometer wires so well, by merely using the Hooke's-joint handles from the circle-screws, as to dispense with a "slipping piece" for the micrometer; and, finally, have never yet seen the smallest trace of instrumental tremor.

Sept. 27, 1862.

Appendix of Results of Observations with the Elchies Telescope in 1862.

28 Aquilæ, Mean R.A. $19^h\ 13^m\ 14^s$, and Decl. + 12° 7′ 22″, Jan. 1, 1862.

Components.	Mags.	Colours.		Position.	Full Weight.	Distance.	Full Weight.
				° ′		″	
A	6·2	Yellowish			= 10·0		= 10·0
B	10·0	Purplish	A B	176 38	(w. 1·0)	60·44	(w. 0·6)
(New) C	17·0	Blueish	A C	102 25	(w. 0·4)	66·76	(w. 0·1)

Date of observation = 1862·72.

The magnitudes and colours in Admiral Smyth's "Cycle" for A and B are "6 and 10, and pale white and deep blue," and are not more different from the above than atmospheri effects might explain, especially as the Elchies Observation were taken during twilight and the influence of its warn colouring. For the position and distance of A and B, previou measures are not abundant; but, numerically, both the angle of position and the distances are consistent in indicating som physical changes. Thus,

	Position.	Distance.	Date.
	° ′	″	
S.	175 4	59·28	1825·04
Sm.	175 42 (w. 8)	59·8 (w. 5)	1831·42 *

The new star therefore, C, picked up by the superic

* This star is not, of course, inserted in Struve's great Catalogue, tl distance of the components exceeding his prescribed limits; but it included in a list (given in the *Mensuræ Micrometricæ*, pp. 317, 318) objects measured with a small instrument in the years 1821–23. Tl results are,

Dist. 60″·01 Pos. angle 176°·9 Epoch 1822·19.—R. G.

optical power of Mr. Grant's telescope, may become an important member in the system for future observers.

δ Aquilæ, R.A. $19^h\ 18^m\ 31^s$, and Decl. + 2° 50′ 25″, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Full Weight.	Distance.	Full Weight.
			° ′		″	
A	3·5	Faint yellow		= 10·0		= 10·0
B	16·0	Blueish	A B 275 35	(w. 0·3)	94·96	(w. 0·2)
C	14·0	Blueish	A C 114 15	(w. 0·3)	192·30	(w. 0·1)

Date of observation = 1862·74.

B was invisible to the Sheepshanks' 5-foot at Guajara, Teneriffe, in 1856; but C was just visible, and, in the total absence of B, and without any micrometrical measures, led to a suspicion of a slip of 180° in the Tables. Now, however, the Elchies telescope, with its greater space-penetrating power, shows B still existing nearly as detailed in the "Cycle," but reduced from the 12th mag. in 1833·62 to the 16th in 1862·74; and if it was of that small size in 1856, no wonder that the small Sheepshanks' telescope was unable to show it.

The changes from the "Cycle" position and distance, 259° 3′ (w. 1) and 96″·5 (w. 1) in 1833·62 are probably effects of proper motion, and for the most part of A; if so, and if B and C, which are on either side of A, be greatly more distant, as indeed their small magnitudes imply, it will be of the utmost importance that A should in future be measured from both of them, instead of from B alone.

128 Anseris, R.A. $19^h\ 20^m\ 26^s$, Decl. + 19° 37′ 12″, Jan. 1, 1862.

Components.	Mags.	Colour.	Position.		Distance.	
			° ′		″	
A	6·5	Orange				
B	13·0	Violet	A B 41 2	(w. 1·0)	22·53	(w. 0·3)
C	12·0	Blue	A C 319 55	(w. 1·0)	68·14	(w. 0·2)

Date of observation = 1862·71.

The existence of the star C was discovered with the Sheepshanks' 5-foot telescope, at Guajara, in Teneriffe, and would appear now to be an important point to observe in future in connexion with A and B; for the above measures, compared with

	Position.		Distance.		Date.
	° ′		″		
Σ	43 35		22·647		1829·40
and Sm.	44 48	(w. 3)	25·0	(w. 1)	1833·58

indicate an orbital movement not hitherto recognised.

The Sm. observation is indeed somewhat anomalous, but it

was taken with an unusual micrometer, and, because "illu nation was precluded to so small an object as B," appeared the Bedford telescope. In Mr. Grant's larger telescope, h ever, B was comparatively so bright that there was no di culty in illuminating the field and employing the usual wi micrometer.

β Cygni, R.A. $19^h 25^m 9^s$, Decl. + 27° 40′ 20″, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Distance.
			° ′	″
A	3	Yellow		
B	7	Blueish-green	A B 55 28 (w. 2·0)	34·20 (w.
(New) C	15·3	Blue	A C 340 2 (w. 1·0)	61·49 (w.
(New) D	15	Blue	A D 33 45 (w. 0·5)	107·50 (w.

Date of observation = 1862·72.

The above magnitudes and colours of A and B coin closely with those in the "Cycle," or, "3 and 7, and topaz yel and sapphire blue." The positions and distances show a sli discordance from the authorities quoted or given there; thus:—

	Position.	Distance.	Date.
	° ′	″	
B	57 34	34·20	1755·00
H	54 52	34·83	1782·45
P	54 31	34·28	1800·00
Σ	54 30	34·29	1821·76
H and S	54 45	34·38	1822·98
D	55 32	34·51	1830·54
Sm.	55 24 (w. 5)	34·2 (w. 5)	1830·81
Sm.	55 36 (w. 9)	34·4 (w. 9)	1837·58

Perhaps the two new stars may assist in proving whet as has been supposed, both A and B are affected by pr motion, and either one differently.

ζ Sagittæ, R.A. $19^h 42^m 52^s$, and Decl. + 18° 47′ 47″, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Distance.
			° ′	″
A	5·2	Whitish		
B	9·7	Violet	A B 313 19 (w. 2·0)	8·58 (w.
(New) C	15·0	Blue	A C 250 42 (w. 1·0)	70·73 (w.

Date of observation = 1862·73.

The above magnitudes and colours again come near t of the "Cycle;" and the positions and distances confirm t

remarkably in a feature where they had differed materially from older authorities, the series running thus:—

	Position.	Distance.	Date.
	° ′	″	
H	304 10	8·83	1781·88
Σ	309 30	8·77	1819·74
H and S	314 32	8·82	1823·69
H	318 26	9·81	1829·63
Sm.	313 24 (w. 7)	8·9 (w. 7)	1831·59
Sm.	312 18 (w. 9)	8·6 (w. 9)	1838·67

The reason of these differences, so much larger than ordinary errors of observation, is difficult to conjecture truly, and may imply a dark planet of large size accompanying one of the stars. The new star C, now shown to exist, is too far off in itself to explain such perturbations, but may prove a useful referring point for the future.

452 Cygni, R.A. $20^h\ 57^m\ 4^s$, and Decl. + 38° 58′ 0″, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Distance.
			° ′	″
A	6	Reddish orange		
B	10	Greenish	A B 300 33 (w. 2·0)	19·22 (w. 0·5)
(New) C	13	Blueish	A C 86 53 (w. 1·0)	98·01 (w. 0·4)
D	15·5	Blueish	A D 138 54 (w. 1·0)	54·29 (w. 0·3)

Date of observation = 1862·73.

The stars A and B are here each one magnitude larger than in the "Cycle;" the colours are nearly the same; but the position and distance indicate rapid proper motion, being given as 297° 0′ (w. 3) and 17″·0 (w. 1) respectively, in 1832·87. Neither C nor D are measured there, but D is evidently alluded to as a star "of the 13th magnitude, *s f*, and sufficiently near to form with A and B a triple object:" a description which indicates a change of magnitude to have occurred since then.

1 Pegasi, R.A. $21^h\ 15^m\ 43^s$, and Decl. + 19° 12′ 56″, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Distance.
			° ′	″
A	4·5	Yellowish		
B	9	Grey	A B 311 16 (w. 2·0)	36·76 (w. 0·5)
(New) C	14·5	Blue	A C 22 59 (w. 1·0)	80·03 (w. 0·3)

Date of observation = 1862·72.

The magnitudes and colours of A and B, as here detailed, are nearly conformable to those of the Cycle; while the angles

position and distances accompanying raise their history from indicating no notable change," in 1833, to exhibiting a regular, ıough slow, orbital movement in 1862; as thus:—

	Position.	Distance.	Date.
	° ′	″	
H	308 19	37·10	1780·69
S	310 11	36·86	1825·22
Sm.	310 48 (w. 8)	36·40 (w. 5)	1833·95

The new star C may or may not form a member of the system, of which the proper motion is so very large that there is much need for a heavenly milestone to measure its movements from.

312 Pegasi, R.A. $21^h\ 45^m\ 6^s$, and Decl. + 19° 10′ 53″, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Distance.
			° ′	″
A	7	Yellowish		
B	15	Blueish	A B 92 28 (w. 1·0)	20·32 (w. 0·5)
(Rediscoverd) C	17	Blueish	A C 323 34 (w. 0·5)	21·74 (w. 0·3)
(New) D	15·5	Blueish	A D 193 48 (w. 0·4)	80·95 (w. 0·2)
(New) E	17	Blueish	A E 355 52 (w. 0·2)	127·30 (?)
(New) F	18	Blueish	A F 308 52 (w. 0·2)	106·10 (?)

Date of observation = 1862·74.

The stars A and B having been described as "in a barren field" when looked at with a good 6-inch object-glass, I turned the Elchies telescope to them, and immediately saw C also; but this star had been previously discovered by Sir John Herschel with his 20-foot reflecting telescope, as mentioned in the "Cycle." A little further attention, however, soon revealed the additional stars D, E, and F, which, as far as I yet know, are new.

The position and distance of A and B, given above, differ from those of the "Cycle" beyond the limits of error of observation, and merit attention therefore; the numbers being

Position.	Distance.	Date.
95°·0 (w. 1)	15″·0 (w. 1)	1838·66

Elchies new (?) R.A. $21^h\ 47^m\ 46^s$, and Decl. + 19° 2′ 53″ nearly, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Distance.
			° ′	″
A	6·7	Yellow		
B	8·7	Lilac	A B 110 58 (w. 2·0)	22·26 (w. 1·

Date of observation = 1862·73.

This pair of stars was picked up accidentally, by its coming into the 5-foot finder of the Elchies grand telescope, when the star "312 *Pegasi*" had passed out, through the weights of the driving clock having run down. I have not yet met with any observations of this double star elsewhere; but my references at the present moment are limited, and it seems too bright and too conspicuous for contrasted colours to have escaped many previous observers.*

ψ' Piscium, R.A. $0^h\ 58^m\ 17^s$, and Decl. + 20° 44′ 1″, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Distance.
			° ′	″
A	5	Yellowish white		
B	5·5	Blueish white	A B 160 22 (w. 2·0)	30·17 (w. 2·0)
C	12·5	Blueish	A C 121 20 (w. 1·0)	97·33 (w. 1·0)

Date of observation = 1862·72.

The existence of C was noted in the "Cycle" as "a very small star following;" but it was not micrometrically observed.

The measures there recorded of A and B are considered "to show *fixity*:" but if the present determination can be trusted to within a very small quantity, an extremely slow orbital movement may rather be inferred; the whole of the previous measures standing thus:—

	Position.	Distance.	Date.
	° ′	″	
♅	170 0	27·50	1779·83
H and S	161 2	30·34	1822·38
Sm.	160 24 (w. 8)	30·2 (w. 8)	1833·97

The mags. and colours, given in the "Cycle" as "both 5·5 mag., silvery white," indicate little relative change in these respects during the last twenty-eight years.

222 Arietis, R.A. $1^h\ 51^m\ 56^s$, and Decl. + 20° 23′ 14″, Jan. 1, 1862.

Components.	Mags.	Colours.	Position.	Distance.
			° ′	″
A	0	0		
B	12	Blueish	A B (w. 1·0)	(w. 1·0)
C	12	Blueish	B C 169 15 (w. 0·5)	36·94 (w. 0·3)
D	7·5	Faint yellowish	B D 0 35 (w. 0·5)	180·44 (w. 0·3)

Date of observation = 1862·74.

The observation of this group gave me great trouble and

* This star appears to be identical with No. 2841 of Struve's great Catalogue (*Mensuræ Micrometricæ*, p. 188).—R. G.

perplexity, until at last, by successive trials, I became perfectly confident of freedom from clouds, and the absolute R.A. and D. in the field of the telescope, and then recognised that every member of the group was present, except its brightest and leading component; viz. A, which the "Cycle" puts down as of the 6th mag.; while B and C and D are entered of the 15th, 10th, and 9th respectively!

Since the year 1834, therefore, D has increased considerably, and B somewhat; while C has slightly decreased, and A gone out altogether, or at least so far as the great Elchies telescope can penetrate into space! Yet perhaps the most immediately important circumstance of the whole case for practical astronomers is, that it has been said of the group, "It forms an admirable test to try the light and distinctness of a telescope."

The "Cycle" positions and distances to compare with the above are,

	Position.	Distance.	Date.
	° ′	″	
A B	53 0 (w. 1)	2·5 (w. 1)	1834·99.
A C	165 0 (w. 2)	40·0 (w. 1)	
A D	359 12 (w. 2)	165·0 (w. 2)	

γ Andromedæ, R.A. $1^h\ 55^m\ 25^s$, and Decl. + 41° 40′ 3″,
Jan. 1, 1862.

Components.	Mags.	Colours.		Position.	Distance.
				° ′	″
A	3·5	Orange yellow			
B	5·5	Blueish green	A B C	62 12 (w. 2·0)	9·73 (w. 0·3)
C	6·0	Blueish green	B C	116 23 (w. 1·0)	0·57 (w. 0·3)

Diameter of disk of B	=	0·37″
,, ,, C	=	0·23
,, ,, wire	=	0·58

Date of observation = 1862·71.

The above observations were procured in the early morning, about sunrise, on one of the only two occasions during the week when I had the fortune to witness, for a few minutes each time, a cessation of those atmospheric disturbances on which the definition of stars seen in a good telescope so greatly depends. The disks of the two close stars were then completely and more than separated, and were so small as to be in diameter only about half the apparent thickness of the fine spider's line of the micrometer employed in the measures.

Concluding Notes on the Observations.

The preceding observations have been hastily arranged, while on the road, so as to represent at one view the results, generally of two and sometimes of three nights' observation of each object. In affixing *weights* to the observations, while following my Father's method, as explained in his "Cycle," and keeping to his upper limit of 10, I have been obliged to descend below his lower limit of 1, by using tenths. This was because the atmospheric perturbations of telescopic definition on the banks of the Spey were really so fearful, and at almost every observation, that, had it not been my first, and too probably my last, opportunity of looking into the Elchies telescope, and therewith examining some anomalous features noted when gazing at certain double stars on the Peak of Teneriffe in 1856, and which I have had no other opportunity of inquiring into, I should not have made any observations at all, but have waited for a more favourable condition of the atmosphere.

By assigning such small weights as I have done, I hope to avoid doing any harm by otherwise sending out inferior, to mix, uncorrected, with superior observations by other and abler hands; while, with some of the objects, the cosmical changes have been so rapid, or the powers of the telescope so transcendent, as to overbear the rudeness of my micrometer-measures, and to show some valuable additions to the many physical facts recorded in the "Cycle" up to its closing epoch. That book is the only one I have yet compared the observations with, and the only one I have with me on the journey; but I trust that I shall be allowed more honourable reasons also, for desiring to use it as a standard work of reference.

The value of the micrometer-screw in seconds of space has as yet only been approximately determined from the observations made for the purpose, and may, perhaps, require a small addition, as of about $\frac{1}{200}$ of the whole; but that present uncertainty on the distances alone, will not at all interfere with the character of the chief results now brought out, as affecting the discovery of new stars and changes in angle of position of old stars, or large variation in their distances.

Illumination of the field, and by red light, according to Sir John Herschel's discriminating suggestion, was always employed for the actual micrometer-measures; but the estimation of magnitudes and colours was always made in a darkened field.

Oban, October 7th, 1862.

Extract of a Letter from Dr. Wolfers to the Rev. R. Main dated Berlin, October 31, 1862.

The xxth volume of the *Radcliffe Observations*, present to me by the Radcliffe Trustees, came into my hands son time ago, and I have compared the places of the fundament stars contained in that volume with those contained in t *Tabulæ Reductionum.* In this manner I have formed t two tables, A and B, belonging respectively to the years 18 and 1860. There are some differences between observati and calculation (F. — W.), which are too great to be attribut to errors of observation. It is possible that there is an err committed in the reduction of the observations, wherefore have included in [the] differences in question. These bei excepted, I have combined the two series of differences wi the series obtained in the year 1861, and published in t *Monthly Notices*, vol. xxii. No. 2, p. 31. In this manner I ha obtained the table designated by the letter C.

Excluding now the R.A. of the circumpolar stars α a γ *Ursæ Majoris*, α, β, and δ *Ursæ Minoris*, and of α *Geminoru* (the latter for a reason previously given), the result of t comparison is the following :—

Number of Observations.	F.—W. in R.A.	Number of Observations.	F.—W. in Decl.
	s		″
448	+0·110	307	+0·032

This result being compared with the result of the precedi year, viz.

Number of Observations.	F.—W. in R.A.	Number of Observations.	F.—W. in Decl.
	s		″
326	+0·017	288	+0·046

it is evident that, as the number of observations is increas the mean difference of the two co-ordinates is diminish These values belong to January 1, 1860.

A.

Right Ascensions and Declinations of Fundamental St observed at the Radcliffe Observatory in the Year 18 compared with those contained in the Tabulæ Reductionu

Name of Star.	Number of Observations.	F.—W. R.A.	Number of Observations.	F.—W. Decl.
		s		″
α Andromedæ	1	0·00		
γ Pegasi	3	−0·11		
α Arietis	1	−0·01		
α Ceti	1	+0·08		
α Geminorum			1	+2·0

Name of Star.	Number of Observations.	F. — W. R.A.	Number of Observations.	F. — W. Decl.
		s		″
α Canis Minoris	1	[+0·15]	1	−1·2
β Geminorum	1	−0·04		
α Hydræ	3	−0·02		
α Leonis	1	0·00		
β Leonis	1	+0·05		
α Virginis	3	0·00	1	−0·1
η Ursæ Majoris			1	−2·7
α Böotis	1	+0·01		
α Coronæ	1	−0·02		
α Serpentis	1	−0·04		
α Herculis	1	0·00		
α Ophiuchi	4	−0·02	1	−0·3
α^1 Capricorni	1	+0·07		
α Cygni	2	−0·04		
α Aquarii	5	−0·03	1	−0·3

B.

Right Ascensions and Declinations of Fundamental Stars, observed at the Radcliffe Observatory in the Year 1860, compared with those contained in the Tabulæ Reductionum.

Name of Star.	Number of Observations.	F. — W. R.A.	Number of Observations.	F. — W. Decl.
		s		″
α Andromedæ	2	[−0·13]		
γ Pegasi	3	[−0·20]		
α Arietis	5	−0·02	1	+2·8
α Ceti	4	−0·05		
α Tauri	1	[−0·27]		
β Leonis	1	+0·05		
β Virginis	2	+0·11		
γ Ursæ Majoris	1	[+0·41]		
η Ursæ Majoris	4	[+0·18]		
α Böotis	1	[−0·38]		
α^2 Libræ	1	+0·06		
β Ursæ Minoris			1	−2·2
α Serpentis	1	+0·11		
α Scorpii	1	[−0·28]		
α Herculis	2	−0·07		
α Ophiuchi	4	−0·04		
γ Draconis	4	+0·12		
α Lyræ	2	−0·03		
γ Aquilæ	9	−0·10	3	−0·6

Name of Star.	Number of Observations.	F.—W. R.A. s	Number of Observations.	F.—W. Decl. ″
α Aquilæ	9	−0·01		
β Aquilæ	11	+0·03	2	−2·0
α² Capricorni	2	+0·13		
α Cygni	14	−0·07		
α Cephei	5	[+0·37]	1	+0·7
β Cephei			4	[−4·6]
α Aquarii	9	−0·06		
α Piscis Austrini	1	−0·01		
α Pegasi	10	+0·02	1	−2·0
α Ursæ Minoris	6	−0·15	3	+0·8
δ Ursæ Minoris			1	+1·5

C.

Right Ascensions and Declinations of Fundamental St observed at the Radcliffe Observatory in the Year 18 *compared with those contained in the Tabulæ Reduction*

Name of Star.	Number of Observations.	F.—W. R.A. s	Number of Observations.	F—W. Decl. ″
α Andromedæ	5	+0·02	5	+0·7
γ Pegasi	12	−0·06	6	+2·6
α Cassiopeiæ	3	+0·09	4	−0·4
α Arietis	12	−0·02	4	+1·8
α Ceti	14	0·00	4	+0·4
α Persei	5	−0·06	3	−0·2
α Tauri	16	−0·02	9	+0·2
α Aurigæ	5	−0·01	6	−0·6
β Orionis	8	0·00	7	+0·4
β Tauri	5	+0·05	6	+2·5
α Orionis	8	+0·02	5	+1·4
α Canis Majoris	8	−0·02	20	−0·4
α Geminorum	10	+0·12	9	+1·8
α Canis Minoris	11	+0·03	10	+0·4
β Geminorum	17	−0·01	10	+0·3
α Hydræ	12	+0·04	7	−2·1
α Leonis	16	+0·01	12	+1·1
α Ursæ Majoris	5	+0·32	12	+0·6
β Leonis	11	+0·07	6	−0·3
β Virginis	11	+0·12	5	−0·4
γ Ursæ Majoris	5	+0·21	2	+0·2
α Virginis	15	+0·06	15	−1·2
η Ursæ Majoris	10	+0·06	3	−1·4
α Böotis	7	+0·02	2	+0·4

Name of Star.	Number of Observations.	F. — W. R.A.	Number of Observations.	F. — W. Decl.
		s		″
α^1 Libræ	1	+0·04	5	−0·4
α^2 Libræ	11	+0·10	3	−0·6
β Ursæ Minoris	1	−0·26	6	−0·5
α Coronæ	10	+0·01	3	−0·7
α Serpentis	12	+0·01	3	−0·4
α Scorpii	12	+0·14	7	−0·2
α Herculis	9	−0·08	1	−0·2
α Ophiuchi	18	−0·02	9	−0·1
γ Draconis	9	+0·07	4	−0·8
α Lyræ	13	+0·05	8	−0·4
γ Aquilæ	15	−0·06	6	−0·7
α Aquilæ	19	−0·03	9	+0·3
β Aquilæ	15	+0·03	6	−0·5
α^1 Capricorni	1	+0·07	2	−1·8
α^2 Capricorni	7	+0·04	5	−2·8
α Cygni	24	−0·05	6	−2·4
α Cephei	11	−0·07	8	+0·9
β Cephei	4	−0·11	10	+0·2
α Aquarii	23	−0·05	3	−0·8
α Piscis Austrini	9	−0·02	8	−0·4
α Pegasi	14	−0·02	7	−0·5
α Ursæ Minoris	14	−0·20	7	+0·3
δ Ursæ Minoris	4	+0·48	10	+1·8

[*Note by Mr. Main.* Of the anomalous results in R.A., referred to by Dr. Wolfers, I may state that the R.A. of *Arcturus*, included in the Catalogue of Stars for 1860, should be rejected, as the number of determining clock-stars is too small. It has crept in by inadvertence. In addition to this, there must have been something singularly wrong in the observation, as will be immediately seen by comparing the clock-error given by it with that given by η *Böotis* (Nov. 21). Of the rest, the R.A.'s of *Aldebaran*, *Procyon*, γ *Ursæ Majoris*, and *Antares*, depend on single observations.

The anomalous cases occurring in 1860, October 11 and November 21, had been scrutinised before the observations passed the press; and the anomalies are due to strange errors in the observations, not to errors in the reductions.

In future years much better means will be given for the continuation of Dr. Wolfers' researches on the places of the fundamental stars, as clock-errors are determined by the same list of stars as that used at Greenwich, and the assumed equinox will be identical with the Greenwich assumed equinox.]

For the following abstract of Mr. Auwers' paper, On the Irregularities of the Motion of *Procyon*, and on the Determination of the Elements of an obscure Companion of *Procyon*, the attraction of which will explain these irregularities, the Society and myself are indebted to W. T. Lynn, Esq., Assistant of the Royal Observatory of Greenwich.

It may be necessary to correct a misunderstanding that might possibly be produced in the minds of some readers by the expressions of the first paragraph. The irregularities in the annual motions of stars generally, and of *Procyon* in particular, were first pointed out by Mr. Pond. In the *Greenwich Observations*, 1820–22, is a Table of "Right Ascensions of the 36 Stars of the Greenwich Catalogue, reduced to the beginning of the year 1819, from five years' observations with the new Transit Instrument, 1817–1821, and compared with Dr. Maskelyne's Catalogue of the year 1805 brought up to the same period;" the principal discordances are those of α^2 *Capricorni*, *Sirius*, *Spica*, *Procyon*. In the *Greenwich Observations*, 1825, is "A Table showing the difference between the predicted and observed N.P.D. of 45 principal fixed stars for the year 1826;" the principal discordances are those of α *Pegasi*, γ *Aquilæ*, *Sirius*, *Procyon*. In the *Philosophical Transactions*, 1823, page 36, Mr. Pond says, "A very great deviation in N.P.D. will be found in three very bright stars, *Capella*, *Procyon*, and *Sirius*;" in page 44 he again specifies *Procyon* and *Sirius*, and in page 529, he speaks of the impossibility of exactly interpolating or predicting a star's place. Finally, in the Introduction to the Catalogue of 1112 stars, dated June 27, 1833, he says, "We can hardly desire a better test of our power of predicting the future position of the stars, than by trying by the same formulæ how accurately we can interpolate for the past. In a variety of papers, which I have submitted at times to the Royal Society, I have endeavoured to show that *with us* the latter experiment entirely fails."

G. B. Airy.

Abstract of the Paper by Auwers on the Proper Motion of Procyon, contained in Nos. 1371, 1372, *and* 1373, *of the Astronomische Nachrichten.*

In this paper Auwers first gives the history of the i vestigations made into the proper motions of *Sirius* and *Procy* thus: Bessel discovered in 1844 that the observed Ri Ascensions of *Sirius* and Declinations of *Procyon* negatived existence of an invariable Proper Motion of those star showed that its variability was to be explained by the att of bodies connected with them, and forming thereby systems, the remaining components of which were n ceptible to us, or at least had not hitherto been per Doubt was on various sides thrown upon this resu'

surprising, and founded as it was upon very small quantities, measurable only by means of very frequently repeated observations, until Peters in 1851 demonstrated that the irregularities in the observed Right Ascensions of *Sirius* were such that they could be completely explained by the assumption of a periodical motion about a neighbouring point, itself in uniform motion; further proof of this explanation has since been afforded by investigation of the observed Declinations of that star. The argument for the existence of a similar variability in the Proper Motion of *Procyon* was greatly increased when Mädler in 1851 showed that irregularities were evidenced in its observed Right Ascensions as well as Declinations. Up to the present time, however, the question, as regards *Procyon*, has been considered an open one; Auwers, therefore, last year (1861) undertook and executed a new investigation of it, the principal features of which are contained in the paper before us, and the result of which has completely, he tells us, confirmed Bessel's hypothesis.

In this investigation he has made use of all the available published observations of *Procyon* from 1750 to 1860, using for stars of comparison in R.A. *α Tauri*, *β*, *α Orionis*, *β Geminorum*, *α Hydræ*, and *α Leonis*; and in N.P.D. *α Ceti*, *α Orionis*, *α Serpentis*, *γ*, *α*, *β Aquilæ*, and *α Aquarii*. The latter list agrees with Bessel's, save that Auwers has omitted *β Virginis*, because it has only exceptionally been observed at the English Observatories. The comparison is effected by comparing the observed place of each star with its computed place in the *Tabulæ Regiomontanæ*. After giving several tables of the results obtained from observations during the above-mentioned times, made principally at Greenwich (Bradley, Maskelyne, Pond, and Airy), Königsberg, Edinburgh, Oxford, Cambridge, Geneva, and Washington, he remarks that inspection alone is sufficient to prove the certainty of changes in the Proper Motion, and therefore he need only occupy himself with the question whether these changes are capable of being explained by the assumption of a motion in a binary system. And he finds that the irregularities are of the nature that they may be explained by the assumption of a circular motion of *Procyon*, in a plane perpendicular to the line of sight, round a point about 1″·2 distant. After trying this assumption and correcting it according to the method of least squares, he at last forms the following set of elements of the motion of *Procyon*:—

		Probable Error.
Epoch of Minimum R.A.	= 1795·5676	± 0y·4457
Yearly Motion	= 9°·00634	0°·09110
Periodic Time	= 39y·972	0y·4043
Radius of Orbit	= 1″·0525	0″·0275

Then follows a Table containing a comparison of these ex-

pressions with observations, which shows "that the representation of the latter by the elements is perfectly satisfactory," there being no remarkable deviation in any part of the orbit, but the differences from observation having the character of being fortuitous, and following no law.

In order to be able to assign a limit to the mass of the invisible companion of *Procyon*, he has deduced a provisional approximate value of the parallax of that star from a series of observations with the Königsberg heliometer, not originally commenced with any view to such a determination. He has thus obtained for that parallax the value 0″·123, with the probable error ± 0″·033; "with this number," he says, "we should find from the above elements that the mass of the dark body must be greater than 0·4 that of the Sun. We may assume with certainty that the parallax of *Procyon* is less than 0″·2; in any case therefore the dark body must have a mass at least a hundred times as great as that of *Jupiter*. If *Procyon* and its companion have equal masses, that of each must be greater than 0·37 that of the Sun, and if the mass of *Procyon* is equal to that of the Sun, that of its companion must be greater than 0·6 that of the Sun."

The paper concludes with a table of the corrections to be applied according to this theory to the Right Ascensions and Declinations of *Procyon* in the *Tabulæ Reductionum* from the year 1750 to 1880.

W. T. L.

Rule for the Solution of Kepler's Problem.
By M. de Gasparis.

(*Communicated by the President.*)

Exemple. Soit $M = 27^\circ\ 12'\ 33''{\cdot}62 = E - (9{\cdot}8947213) \sin E$.

Je pose $A = 9{\cdot}3471561$, qui s'obtient en otant log. *e* du nombre 9·2418774, qui est le logarithme de 10° en parties du rayon.

Après cela l'on prend les log $\frac{\sin(M+10^\circ)}{1}$, log $\frac{\sin(M+20^\circ)}{2}$, log $\frac{\sin(M+30^\circ)}{3}$, etc., et l'on s'arrète lorsque le résultat est proximément $> A$.

Le but de cette opération est d'approcher E à 10° près. suffit donc d'envisager les premiers chiffres, et pourvu que sache les premiers chiffres des logarithmes des nombres simp

l'opération se fait à vue d'œil. L'on trouve E $=67^\circ\ 12'$ à 10° près. Et E—M= * 4 dixaines de degré. L'on poursuit ainsi

Log sin $67^\circ\ 12' = 9{\cdot}965$	Log $77^\circ\ 12' = 9{\cdot}989$
Log * $4 = 0{\cdot}602$	Log $5 = 0{\cdot}699$
$9{\cdot}363$	$9{\cdot}290$

Maintenant pour 73 différence entre ces deux derniers logarithmes, la variation de E est $10 \times 1^\circ$; pour 16, différence entre 9·363 et A, la variation de E est * $2 \times 1^\circ = 2^\circ$. Donc E$=69^\circ\ 12'$ a 1° près.

Log sin $69^\circ\ 12' = 9{\cdot}9707$	Log sin $70^\circ\ 12' = 9{\cdot}9735$
Log * $42 = 1{\cdot}6233$	Log $43 = 1{\cdot}6335$
$8{\cdot}3474$	$8{\cdot}3400$

$74 : 10 \times 6' :: 3 :$ * $0 \times 6' = 0'$ Donc E$=69^\circ\ 12'$ à $6'$ près,

ici l'on a pris la différence de 8·3474 avec A—1.

Log sin $69^\circ\ 12'\ 30'' = 9{\cdot}97075$	Log $69^\circ\ 18'\ 30'' = 9{\cdot}97104$
Log * $420 = 2{\cdot}62325$	Log $421 = 2{\cdot}62428$
$7{\cdot}34750$	$7{\cdot}34676$

$74 : 10 \times 36'' :: 34 :$ * $4 \times 36'' = 2'\ 24''$ Donc E$=69^\circ\ 14'\ 54''$ à $36''$ près,

ici l'on a pris la différence de 7·34750 avec A—2, et l'on a pris le log sin $69^\circ\ 12'\ 30''$ au lieu de log sin $69^\circ\ 12'$ pour tenir compte des secondes comprises dans M.

L'on doit remarquer que l'on a approché E à $36''$ sans avoir du tenir compte des parties proportionnelles des logarithmes pris dans les tables. On pourrait dire de même jusqu'à l'approximation à $3''{\cdot}6$.

Log sin $69^\circ\ 14'\ 57'' = 9{\cdot}970872$	Log sin $69^\circ\ 15'\ 33'' = 9{\cdot}970900$
Log * $4204 = 3{\cdot}623663$	Log $4205 = 3{\cdot}623766$
$6{\cdot}347209$	$6{\cdot}347134$

$75 : 10 \times 3''{\cdot}6 :: 53 :$ * $7 \times 3''{\cdot}6 = 25''{\cdot}2$ Donc E$=69^\circ 15' 22''{\cdot}2$ à $3''{\cdot}6$ près.

* It is to be noticed that the proportions which give the numbers which the author has thus marked, with an asterisk, depending on a multiplication by 10, and a division where the quotient is taken to a single figure only, are solved at sight.—Ed.

Log sin 69°15′22″·82 = 9·9708926	Log sin 69°15′26″·42 = 9·9708955
Log ✱ 42047 = 4·6237350	Log 42048 = 5·6237453
5·3471576	4·3471502
74 : 10 × 0″·36 :: 15 : ✱ 2 × 0″·36 = 0″·72	E = 69°15′23″·54 à 0″·36 près.
Log sin 69°15′23″·54 = 9·9708932	Log sin 69°15′23″·90 = 9·9708935
Log 420472 = 5·6237371	Log 420473 = 5·6237381
4·3471561	4·3471554
7 : 10 × 0″·036 :: 0 : ✱ 0 × 0″·036 = 0	E = 69°15′23″·54 à 0″·01 près.

Pour les grandes excentricités la différence, qui dans cet exemple est 74, est proximément constante. Cela peut servir pour controle dans les calculations.

Enfin, il est plus long d'expliquer cette règle, que de la mettre en pratique.

Naples, 21 *Mai*, 1862.

Observations of the Planet Neptune, made with the Olcott Meridian Circle, at the Dudley Observatory, Albany, U.S.A. By G. W. Hough, Assistant.

(*Communicated by Prof. Mitchell through the Astronomer Royal*).

1862.	M.T. Dudley Observatory. h m s	App. R.A. h m s	App. Decl. ° ′ ″
Sept. 24	11 46 54·61	.....	−1 13 44·76
25	11 42 52·65	0 2 45·70	1 14 25·34
28	11 30 46·79	0 2 27·51	1 16 25·03
Oct. 3	11 10 37·29	0 1 57·46	1 19 41·17
8	10 50 28·42	0 1 28·05	1 22 51·17
9	10 46 26·66	0 1 22·18	
14	10 26 18·78	0 0 53·76	1 26 29·20
28	9 30 4·42	23 59 41·92	1 33 59·68
29	9 26 3·94	23 59 37·34	1 34 27·89
Nov. 18	8 6 12·93	23 58 24·31	1 41 45·60
19	8 2 14·52	23 58 21·81	1 42 0·50
22	7 50 19·74	23 58 14·74	1 42 38·27
25	7 38 25·94	23 58 8·67	1 43 10·80
26	7 34 28·37	23 58 7·0	1 43 20·93
30	7 18 38·92	23 58 1·18	1 43 46·47
Dec. 2	7 10 44·88	23 57 58·96	1 43 56·49
3	7 6 48·04	23 57 58·02	1 43 58·1
14	6 23 31·28	23 57 56·39	1 43 42·
17	6 11 45·83	23 57 58·59	1 43 20·
18	6 7 50·91	23 57 59·59	1 43 11·
21	5 56 6·92	23 58 3·34	1 42 3·
24	5 44 24·05	23 58 8·21	1 41
	5 21 1·61	23 58 21·28	−1 40

Date.	R.A. Correction to Amer. Eph.	R.A. Correction to Naut. Alm.	N.P.D. Correction to Amer. Eph.	N.P.D. Correction to Naut. Alm.
1861.	s	s	′	″
Sept. 24	...	...	+10·41	+4·98
25	−1·63	−0·81	11·09	5·68
28	1·60	0·80	11·68	6·23
Oct. 3	1·56	0·78	12·23	6·76
8	1·50	0·71	11·97	6·43
9	1·57	0·79		
14	1·63	0·88	11·40	6·17
28	1·56	0·81	9·94	4·52
29	1·53	0·78	9·64	4·23
Nov. 18	1·53	0·84	10·40	5·41
19	1·47	0·78	10·80	5·87
22	1·52	0·81	9·73	4·89
25	1·59	0·89	10·44	5·61
26	1·50	0·81	11·57	6·71
30	1·48	0·79	9·11	4·20
Dec. 2	1·51	0·82	10·00	5·09
3	1·54	0·82	8·36	3·53
14	1·49	0·85	11·14	6·47
17	1·50	0·85	10·93	6·13
18	1·49	0·85	11·53	6·74
21	1·48	0·86	9·30	4·60
24	1·47	0·87	10·80	6·13
30	−1·51	−0·91	+10·83	+6·11

	R.A. Mean Correction to Amer Eph.	R.A. Mean Correction to Naut. Alm.	N.P.D. Mean Correction to Amer. Eph.	N.P.D. Mean Correction to Naut. Alm.
	s	s	′	″
Sept. and Oct.	−1·57	−0·79	+11·04	+5·63
Nov.	−1·52	−0·82	+10·17	+5·45
Dec.	−1·50	−0·85	+10·36	+5·60
Mean Day. Nov. 17	−1·529	−0·824	+10·52	+5·56

From these results it is readily seen that neither Ephemeris represents the motion of the planet correctly in R.A. The variation for 100 days is $+0^s{\cdot}109$ for the American Ephemeris, and $-0^s{\cdot}087$ for the *Nautical Almanac*. By applying the variation, the probable error for a single observation is $0^s{\cdot}029$ for the American Ephemeris, and $0^s{\cdot}025$ for the *Nautical Almanac*.

In Declination the probable errors are $0''{\cdot}80$ and $0''{\cdot}79$. The transits on October 28th, November 19th, and December 2d, were observed on 9, 10, and 7 wires respectively. In all the other observations 15 wires were used, and the times of transit recorded in on the Chronograph. Any change in the rate of the Clock (during the time of observation) was satis-

factorily determined by comparison with two other Clocks, by causing their pendulums to make records on the Chronograph at intervals of 30 minutes.

In converting the chronographic records into time, every wire was measured to the hundredth of a second. But in the "mean" and all subsequent reductions the thousandths were retained, being rejected in the final result only.

N.B. The observations published in the *Monthly Notices*, vol. xxii., No. 1, pages 18 and 19, were not corrected for parallax.

Results of the Meridional Observations of Small Planets; Occultations of Stars by the Moon; and Transit of Jupiter's Third Satellite; observed at the Royal Observatory, Greenwich, from June to September, 1862.

(*Communicated by the Astronomer Royal.*)

Hebe ⑥.

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, June 14	10 57 12·3	16 29 6·40	90 4 8·51
16	10 47 32·6	16 27 18·25	90 9 8·15
27	9 55 27·9	16 18 27·14	90 51 1·69
28	9 50 50·7	16 17 45·68	90 55 57·26
July 2	9 32 34·3	16 15 12·55	91 17 34·97

Iris ⑦.

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Aug. 18	13 33 11·0	23 21 26·92	83 8 41·41
19	13 28 43·3	23 21 15·01	83 8 2·01
27	12 52 6·6	23 16 4·78	83 11 48·47
28	12 47 26·2	23 15 20·15	83 13 28·23
Sept. 3	12 19 3·5	23 10 32·15	83 28 30·87
10	11 45 29·2	23 4 28·13	83 56 38·05
11	11 40 40·6	23 3 35·32	84 1 28·79
16	11 16 39·9	22 59 13·45	84 28 14·24
17	11 11 52·9	22 58 22·24	84 34 1·23
19	11 2 20·9	22 56 41·71	84 46 0·53
22	10 48 18·9	22 54 17·04	85 4 48·02
23	10 43 26·8	22 53 30·75	85 11 12·95
25	10 34 6·3	22 52 1·86	85 24 12·32

Parthenope (11).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, July 12	13 14 56·0	20 37 36·38	107 42 16·40
19	12 41 44·7	20 31 55·46	108 22 12·61
25	12 12 50·4	20 26 35·74	108 58 3·46
26	12 8 0·1	20 25 41·28	109 4 5·79
31	11 43 49·0	20 21 8·92	109 33 48·05
Aug. 14	10 37 20·2	20 9 41·02	110 49 0·21
25	9 47 58·0	20 3 32·82	111 34 16·79
29	9 30 52·9	20 2 11·12	111 46 55·66

Egeria (13).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Sept. 3	13 13 57·2	0 5 34·78	112 56 10·43
16	12 9 59·9	23 52 42·22	113 39 0·57
22	11 40 10·8	23 46 27·48	113 46 56·83

Melpomene (18).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, June 4	9 45 33·4	14 37 50·13	89 43 58·59

Pomona (32).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Sept. 16	12 50 23·2	0 33 12·16	81 2 57·88
17	12 45 41·7	0 32 26·45	81 8 50·69
22	12 22 6·3	0 28 29·95	81 39 31·59
25	12 7 52·5	0 26 3·41	81 59 10·24

Lætitia (39).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, June 30	12 18 37·6	18 53 50·02	99 6 56·01
July 12	11 21 13·5	18 43 35·15	99 47 53·86
19	10 48 5·8	18 37 57·94	100 18 55·68

Aglaia (47).

Mean Solar Time of Observation.	h m s	R.A. from Observation. h m s	N.P.D. from Observation. ° ′ ″
1862, Sept. 22	12 48 2·2	0 54 30·10	83 23 40·08

Doris (49).

Mean Solar Time of Observation.	h m s	R.A. from Observation. h m s	N.P.D. from Observation. ° ′ ″
1862, July 19	12 28 6·5	20 18 15·04	101 3 11·11
31	11 31 50·3	20 9 8·23	101 42 25·34
Aug. 7	10 59 14·1	20 4 2·63	102 8 7·76
19	10 4 31·3	19 56 29·43	102 53 33·09
22	9 51 12·9	19 54 58·60	103 4 57·07
25	9 37 58·4	19 53 31·61	103 15 57·14

All the observations of N.P.D. are corrected for refraction and parallax.

Occultations of Stars by the Moon.

Day of Obs.	Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
1862, July 15	κ Piscium reapp. (*a*)	Dark	11 25 20·6	E.
15	9 Piscium reapp. (*a*)	Dark	11 30 22·8	E.
21	υ^1 Tauri reapp.	Dark	14 56 59·3	T. C.
Sept. 3	π Sagittarii disapp.	Dark	11 45 23·2	C.

(*a*) Observed through thin cloud; considered true to a second.

The initials E., C., and T. C., are those of Mr. Ellis, Mr. Criswick, and Mr. Chappell.

The egress of *Jupiter's* third Satellite was observed on June 16, by Mr. Ellis, to take place at $10^h\ 22^m\ 17^s\ 2$, M.S.T.

Measures of the Planet Mars, made, at the Opposition of 1862, *with the Heliometer at the Radcliffe Observatory, Oxford, for the Determination of the Ellipticity of the Disk.* By the Rev. R. Main. (Abstract.)

This paper, which is accompanied by eleven drawings of the planet *Mars*, gives the results of eighteen sets of measures made by Mr. Main for the purpose of determining the ellipticity and size of the planet. Each set of observations consists of at least ten measures of the polar, and ten of the equatoreal diameter, and each result is rigorously corrected for defective

illumination of the disk in the direction of each diameter, and for the corresponding effects of refraction.

The results arrived at are as follows :—

Day, 1862.		Corrected Equatoreal Diameter.	Corrected Polar Diameter.	Ellipticity.		Equatoreal Diameter at Unit of Distance.
Sept.	18	22″·386	21″·872	0·02296	$\frac{1}{43·55}$	9″·25
	19	22·987	22·374	·02667	$\frac{1}{37·50}$	9·47
	22	22·978	22·729	·01084	$\frac{1}{92·28}$	9·39
	23	22·914	22·162	·03282	$\frac{1}{30·47}$	9·34
	25	23·112	22·576	·02319	$\frac{1}{43·12}$	9·39
	27	23·131	22·543	·02542	$\frac{1}{39·34}$	9·39
	30	23·018	22·924	·00408	$\frac{1}{244·87}$	9·35
Oct.	8	22·674	22·208	·02055	$\frac{1}{48·66}$	9·38
	9	22·780	22·314	·02046	$\frac{1}{48·88}$	9·46
	11	22·564	22·230	·01480	$\frac{1}{67·56}$	9·45
	15	21·734	21·385	·01606	$\frac{1}{62·28}$	9·31
	16	21·668	21·296	·01717	$\frac{1}{58·25}$	9·34
	17	21·659	20·938	·03507	$\frac{1}{28·51}$	9·42
	18	21·495	20·828	·03103	$\frac{1}{32·23}$	9·39
	20	21·034	20·775	·01231	$\frac{1}{81·21}$	9·33
	22	20·830	20·419	·01973	$\frac{1}{50·68}$	9·39
	23	20·774	20·267	·02556	$\frac{1}{39·12}$	9·44
	26	19·942	19·389	·02773	$\frac{1}{36·06}$	9·31
Means		.....	.....	0·02147	$\frac{1}{46·58}$	9·38

The ellipticity requires correction for the depression of the Earth below the plane of the equator of *Mars*. When this correction is applied, the ellipticity is finally $\frac{1}{39·04}$.

The angular diameter at the unit of distance, namely, 9″·38, corresponds to a real diameter of 4332 miles, but this still requires correction for the effect of irradiation. For the determination of the irradiation, Mr. Main proposes to make observations when the disk of *Mars* has become small.

Places derived from Observations of Comet II., 1862, *made with the Heliometer at the Radcliffe Observatory, Oxford.* By the Rev. R. Main.

Day, 1862.	Greenwich Mean Solar Time.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
Aug. 5	9 47 4·0	6 22 30·07	13 54 8·5
"	10 50 33·6	6 22 47·52	13 52 5·7
7	10 7 18·7	6 41 39·76	12 27 27·6
9	9 29 3·4	7 9 12·24	10 58 10·3
"	9 47 36·3	7 9 29·07	10 57 35·2
"	10 17 8·5	7 9 46·80	10 56 46·9
"	10 31 9·2	7 9 55·67	10 56 8·0
"	10 40 23·4	7 10 2·31	
"	10 48 14·9	7 10 9·44	10 55 46·4
"	11 1 23·2	7 10 18·41	10 55 19·0
14	8 59 7·2	9 42 3·32	8 0 3·3
18	10 55 26·9	12 57 38·98	10 55 12·0
19	11 21 32·6	13 31 51·60	13 13 46·0
22	9 30 26·2	14 35 49·24	21 2 46·9
"	9 49 40·7	14 36 1·31	21 5 3·1
"	10 13 21·2	14 36 16·01	21 8 29·0
"	10 27 41·9	14 36 25·30	21 10 20·3
23	9 47 30·1	14 49 59·60	24 41 36·5
25	9 53 43·8	15 12 9·44	33 14 38·6
29	8 51 22·9	15 36 43·24	54 37 19·8
"	9 11 38·1	15 36 46·05	54 42 30·9
"	9 37 15·2	15 36 52·64	54 48 51·8
"	9 50 20·5	15 36 54·11	54 52 3·7
"	10 8 10·4	15 36 57·17	54 56 17·8
"	10 31 30·1	15 37 2·18	55 1 46·9

Assumed Mean Places of the Stars employed for Comparison.

Day of Obs.	Star's Name.	Mean R.A. 1862, Jan. 1.	Mean N.P.D. 1862, Jan. 1.
		h m s	° ′ ″
Aug. 7	B.A.C. 2210	6 39 53·16	12 51 22·0
9	B.A.C. 2210		
"	Radcliffe 1928	7 11 28·81	11 2 20·5

Day of Obs.	Star's Name.	Mean R.A. 1862, Jan. 1.	Mean N.P.D. 1862, Jan. 1.
		h m s	° ′ ″
Aug. 14	Groomb. 1643	10 21 35·42	8 47 50·2
18	B.A.C. 4527	13 25 49·48	10 38 35·1
19	Oeltz. Arg. 13813	13 30 39·48	13 13 45·5
22	Groomb. 2129	14 28 33·63	21 18 31·1
25	Groomb. 2211	15 10 2·56	33 21 57·2
29	Piaz. xv. 148	15 33 28·78	54 52 20·2

The observations in general consisted of distinct readings of the hour-circle, and the declination circle corresponding to distinct transits and bisections of the Comet, and the comparison-stars, except on August 9, when the transits of the star and Comet were made with an unchanged position of the polar axis, the objects being in the field of the telescope together.

The results on August 5 and 23 are more doubtful than the rest through want of a comparison-star.

The observations to August 14, inclusive, were made by myself; after that time they were made by Mr. Quirling.

Observations of Comet II. 1862, *taken with the Equatoreal of the Liverpool Observatory.* By J. Hartnup, Esq.

1862.	G.M.T.	Comet's R.A.	Log. $\frac{p}{P}$	Comet's N.P.D.	Log. $\frac{q}{P}$	Stars of Comp. B.A.C.
	h m s	h m s		° ′ ″		
Aug. 18	8 11 55·9	12 53 13·87	+9·309	10 48 14·0	+8·2211	4527
18	8 57 3·3	12 54 25·25	+9·323	10 51 20·6	−8·9740	,,
18	9 42 8·7	12 55 36·51	+9·321	10 54 28·6	−9·3168	,,
21	9 57 5·7	14 18 44·38	+9·107	17 55 10·8	−9·3093	4909
21	10 37 31·9	14 19 17·06	+9·110	18 0 14·4	−9·3688	,,
23	8 11 41·9	14 49 11·09	+8·864	24 26 13·6	+8·1605	4949
23	8 26 47·7	14 49 19·24	+8·887	24 28 41·1	−7·7688	,,
23	8 41 54·3	14 49 26·93	+8·905	24 31 4·4	−9·3188	,,
28	9 12 11·0	15 31 41·35	+8·656	48 53 23·1	−9·6022	5122
28	9 32 12·7	15 31 45·58	+8·676	48 58 11·9	−9·6345	,,
28	9 52 14·6	15 31 50·10	+8·693	49 3 1·1	−9·6655	,,
Sept. 10	10 27 23·6	16 8 0·19	+8·514	108 46 30·4	−9·9433	6060

The observations are corrected for refraction. The corrections to be pplied for parallax in time, and are represented by p and q. P is the equatoreal horizontal parallax.

The following are the assumed mean places of the stars of comparison for 1862, January 0:—

	R.A.	N.P.D.	Authority.
	h m s	° ′ ″	
B.A.C. 4527	13 25 49·25	10 38 35·39	British Association Catalogue.
,, 4909	14 45 7·31	17 27 30·68	,, ,, ,,
,, 4949	14 55 24·07	23 31 2·37	,, ,, ,,
,, 5122	15 25 58·19	48 41 38·95	,, ,, ,,
,, 6060	17 47 47·94	108 46 23·90	,, ,, ,,

Remarks.

Aug. 18. Nucleus stellar, small and well defined. Tail in large telescope fan-shaped, short, and very bright. In a good opera-glass the tail appeared about 8 degrees long.

23. In the large telescope, with powers 90, 120, and 400, the nucleus was elongated, well defined, small, and bright. In the finder (power 20) the nucleus appeared round, and resembled a star of the 5th magnitude, surrounded with faint nebulous light.

28. Nucleus not so bright as on the 23d, but still well defined and a little elongated. Light of comet more diffused.

Sept. 10. Comet found with an opera-glass. In the large telescope the light was much more diffused than on previous occasions.

Observations of Comet II., 1862. By G. Knott, Esq.

The late fine (and in some respects remarkable) Comet was first seen at this Observatory on August 18th. Unfortunately I was a good deal away from home during its apparition; and this circumstance, coupled with the unsettled state of the weather, prevented my observing it so closely as I should otherwise have done.

At the suggestion of the Rev. T. W. Webb, I measured, or carefully estimated, whenever opportunity offered, the position-angle of the curious fan or jet which streamed from the nucleus; and although I am not inclined to attach much weight to my results, I think they abundantly suffice to show remarkable variations in the direction of the fan, to the extent of more than 50°. The measures were made with a wire micrometer attached to my 7½-inch Equatoreal, the power employed being 113; and in all cases, unless otherwise stated, the angle is that of a central chord of the jet.

I proceed to give a few extracts from my journal:—

"August 18th. My first view of the comet in the Equatoreal. The nucleus is bright and sharply defined; bears magnifying well. A curious ray or jet proceeds from it, having a position-angle of 280° or 285°.

"August 25th. The nucleus evidently not in the centre of the coma. The jet is much curved. A tangent to the jet near the nucleus would have a position-angle of 235° or 240°.

"August 27th. A fine evening. With the naked eye the tail can be traced for about 20°. In the telescope the tail appears to be double. The inner edge of the longer and brighter branch is sharply defined; one measure gave its initial position-angle =75°. The southern branch short and ill-defined. At $8^h\ 45^m$ G.M.T. five measures gave the position-angle of jet =241°·5. Later in the evening, $10^h\ 40^m$, the position-angle deduced from five measures was 240°·8; approximate length of jet, 46″±.

"August 29th. Apparently an extraordinary change in the form of the jet. The nucleus very bright, sharply defined, and of an orange tint. The position-angle deduced from five measures at $8^h\ 30^m$ was 279°·0. Clouds now came up, and further observation was impossible.

"August 30th. The jet appears to have recovered its usual form. The nucleus and jet not well defined; the position-angle of the latter at $9^h\ 30^m$ estimated at 225°. Later in the evening took measures: the mean of five gave the angle at $10^h\ 7^m$ =225°·6. This was the last occasion on which I saw the Comet."

The marked changes in the position-angle of the jet are sufficiently curious, and I much regret that circumstances did not allow me to obtain a more complete series of measures. I can only hope that other observers have been more fortunate.

Woodcroft Observatory, Cuckfield, Nov. 11, 1862.

Observations of Comet II., 1862. By F. Abbott, Esq.

I herewith send you the approximate positions of a fin Comet, which first made its appearance here at the early pa of the month. From the 1st to the 6th the sky was so u usually cloudy as to entirely shut out the Moon's place, a also from the 6th to the 11th, from the latter date it observed every evening with one exception, the results chiefly obtained with the first trial of a Dallmeyer's por Equatoreal, just imported—no correction having been ma instrumental error or refraction — the positions, howeve ety. generally taken near the zenith.

The small sketch enclosed, taken on the 11th, will some idea as to the Comet's general appearance. On t was distinctly seen in a clear sky with a Moon thir old. On the 11th some measures were made on its c

about 5° in length, 40′ in diameter, with a planetary nucleus of 20″; on this evening δ *Scorpii* was seen doubly through the tail clear and well defined. On the evening of the 12th the condensed head of the Comet covered the small nebula marked 80 *Messier*, which nebula could not be seen at this time by any means I could adopt. On the 21st the Comet had apparently diminished so much as to be just visible to the naked eye when pointed out, and on the 22d it became telescopic.

		R.A.	Decl.
	h m	h m	° ′
1862, Sept. 6	6 30 P.M.	15 59	4 0 S.
11	8 30 ,,	16 10	19 40
12	7 20 ,,	16 11	22 30
14	10 10 ,,	16 13	26 25
15	7 10 ,,	16 14	27 30
16	10 30 ,,	16 15	29 20
17	7 35 ,,	16 16	31 10
18	7 15 ,,	16 17	32 0
19	7 35 ,,	16 18	33 30
20	7 45 ,,	16 19	34 30
21	7 30 ,,	16 20	35 50 S.

Private Observatory, Hobart Town, Sept. 23, 1862.

On the Cluster κ Crucis, R.A. 12^h 43^m 36^s, *N.P.D.* 149° 25′ 31″ (3435, *H.*) *Lac.* 1110 (*Neb.*). By F. Abbott, Esq.

This delightful cluster, "which is estimated by Sir J. Herschel to be composed of from 50 to 100 stars," most of which partake of well-marked and varied colours, forms an object that is scarcely perceptible to the naked eye, but when under proper optical influence it is one of the most brilliant and interesting objects in the Southern sky.

This cluster is not only an object of interest from the extreme beauty of colour and arrangement, but with respect also to certain changes that are apparently taking place in the number, position, and colour of its component stars.

Some hesitation might be felt in following the author of the *Cape Observations*, with the means he employed, were it not for the encouraging invitation that is given for other observers to note any remarkable change that may have taken place since those results were published. Having, therefore, no knowledge of any other observation being made, or popular

account published of *κ Crucis*, except that at Feldhausen, I have adopted it for comparison with the observations now made, and given in the drawing for the present epoch.*

The colour of all the stars, where distinct colour could be detected, is given on the drawing; the smaller stars, however, from the 10th to the 14th magnitude, are generalised, and all partake of nearly the same colour (Prussian blue), some with a little more or less tint of red or green mixed with the blue. The same Greek letters have, with one exception, been used in the drawing as those used for the Cape Monograph, but not exactly following those used in the Catalogue. The letters and numbers, when in combination, are grouped together in brackets, and intended to show colour and position only.

The 75 stars which are given in the drawing were observed, and their position laid down by means of a 5-foot achromatic telescope, with a $4\frac{1}{4}$-inch object-glass of excellent quality. The power used for the purpose of laying down the position of the stars was 135; but for the colours a comet eyepiece of 27 was found preferable.

The colours, as well as the positions, were afterwards checked with a 7-foot achromatic by Dollond. The evening of the 24th of May was chosen for confirmation: it was a capital night — no moon, quite calm, and the object near the zenith; but with such a night I was not, with the means employed, able to bring out stars of the 15th and 16th magnitude given in the Cape Catalogue.

In the *Cape Observations* φ is laid down to the west of ϵ and δ; they are now, however, situated in a straight line, which, when continued, reaches the star ζ; a straight line also, drawn through α and β, cuts δ; but the two conspicuous stars in the drawing, ν and θ, as well as the three small stars marked 12 above the belt, are not shown at all in the Cape Monograph; there are also two considerable stars, κ and λ, to the far west, which are not seen in the Cape description.

The two stars, α and β, apparently retain their colour; but ν has changed from greenish white to blueish purple; δ from green to pale cobalt; ϵ from red to Indian red; ζ from green to ultramarine; φ (marked η in the drawing) from blue green to emerald green; α^2, called ruddy, partakes now of much the same colour as all the small stars of that magnitude.

Private Observatory, Hobart Town,
May 1862.

* The drawing was exhibited at the November Meeting of the Society. — Ed.

On the Identity of the Triple Star H I. 13.
By the Rev. W. R. Dawes.

This object is thus described by Sir W. Herschel in his first Catalogue of Double Stars:—

"July 25, 1781. In Constellatione *Aquilæ*, near Fl. 37. A curious treble star. It is the last star of a telescopic trifolium n. following *k*, similar to that in the hand of *Aquarius*. The two nearest very unequal; the third star excessively small, and not visible with 227. The two nearest with 460, no more than half diameter of L; the farthest about 7″ or 8″."

The two nearest were measured by Sir James South at Passy in July and August 1825, with his 7-foot refractor of 5 inches aperture; but on the night of the 28th of July, which he describes as "very favourable for difficult observations," he records, "Sir W. Herschel having described it as a triple star, I applied a power of 413; but no third star could be even suspected." It is distinguished as S 720.

In the Dorpat Catalogue of 1827, Struve has thus described his No. 2541:—"2541, P. xix. 185; $\alpha = 19^h\ 27^m{\cdot}3$; $\delta = -10^\circ\ 48'$; I. (9) (10.11) = H I. 13." And his No. 2545 is described thus:—"2545, $\alpha = 19^h\ 29^m{\cdot}0$; $\delta = -10^\circ\ 32'$; I. (6) (8)."

In his "Adnotationes" at the end of the Catalogue, he remarks, with reference to "No. 2541, P. xix. 185. Hanc stellam H I. 13 posui. Est quidem egregia altera duplex classis I. in vicino, No. 2545. Sed hæc est stella magnitudinis sextæ, qualem *Herschelius* non videtur observasse. Neutram tamen triplicem vidi, qualis H I. 13 esse debet."—In his *Mensuræ Micrometricæ*, p. 80, he gives three nights' measures of No. 2541; and at p. 53, five nights' measures of No. 2545. Attached to the former is the remark,—"Dubitari potest utrum hæc stella an 2545 sit Herscheli I. 13."—Though No. 2545 was observed on five nights, on three of which power 480 was used, and on one 600, yet no third star was ever perceived.

There is a second triple star in this vicinity, No. 2547 of Struve's Catalogue. Its Right Ascension is about $12\frac{1}{2}^s$ greater than that of 2545, and it is about 11′ further south. This was observed by Struve on three nights, but *only as double*.

Sir John Herschel reobserved S 720 on three nights in 1828 and 1830. (Nos. 553–555 of the 364 Double Stars. *Mem. R. Ast. Soc.* vol. v. part i. p. 69). In one of his notes at the foot of the page he remarks, "Taken for Σ 2545;" and in another, "This is S 720. It cannot be H I. 13." *Why*, is not stated. No third star was seen on any occasion. Sir John also observed a double star which he designates Σ 2545 (No. 556 of the 364); but the angle of position differs so widely from Struve's, that either there is a large error in it, or another object has been observed, which, however, I have failed to find. No. 557 is rightly called Σ 2547, and the

position and distance agree well with Struve's; but the third star has escaped Sir John as well as Struve.

In 1835, Admiral Smyth observed two objects in this locality, which constitute Nos. 702 and 703 of the "Bedford Cycle." The former is marked as *triple;* the latter as *double only.* No. 702 is thus described:—"A delicate triple star, on the right knee of *Antinous*. . . . A 9, light yellow; B 10, cerulean blue; C 12, violet tint; and the three lie nearly in a line pointing to a distant small star in the *s. f.* quadrant."—It is stated to be P. xix. 185, and to be identical with H I. 13, with S 720, and with Σ 2541,—the positions and distances of those stars, as determined by the respective observers, being quoted for comparison. With reference to the third star overlooked by Struve and South, he adds, "It is plain enough in my refractor. I therefore conclude Sir James must have measured the following instead of 13 H I." Smyth gives as the results of his own observations of 702:—

A B	P = 338·4 (*w*. 4)	D = 3″·2 (*w*. 2)	Epoch 1835·58
A C	153·5 (*w*. 2)	8·0 (*w*.)	

And the mean place of the star for 1840 is given thus:—

R.A.	$19^h\ 28^m\ 1^s$	Prec.	$+\ 3^s{\cdot}30$
Decl.	S. 10° 46′·8		— N. 7″·51

Of the Cycle No. 703, Smyth gives as the mean place for 1840:—

R.A.	$19^h\ 28^m\ 5^s$	Prec.	$+\ 3^s{\cdot}30$
Decl.	S. 10° 30′·4		— N. 7″·52

and he considers it to be identical with P. xix. 186.

As the results of his measurements of this star he gives:—

P = 315°·7 (*w*. 6) D = 3″·8 (*w*. 3) Epoch 1835·58;

and he describes the object thus:—"A very neat double star on the right knee of *Antinous*, and closely following the above-described object. A 7½, pale white; B 9, sky-blue; and they are followed by another double star nearly on the parallel. There can be little doubt of this object's having been measured for 185 P.; for, though it is considerably brighter and more open, yet there is certainly a great resemblance in the position and distance of A and B. This is the No. 2545 of the great Dorpat Catalogue."

Wishing to determine, if possible, which of the two objects in this vicinity at all resembling H I. 13, was really that star, in October 1859 I directed toward the place my 8¼-inch re-

fractor. Having identified 37 (*k*) *Aquilæ*, which is, in fact, a good fifth magnitude star, and plainly visible to the naked eye, I carefully examined all the stars north-following 37 which were visible along with it in the finder of 2 inches aperture and power 23. The nearest double star to 37, and, with reference to that star, about 8′ 35″ n. f. 2^m 12^s, was obviously Σ 2541; the second was about 24′ 44″ n. f. 4^m $7^s{\cdot}5$, and agreed in place with Σ 2545; and a third was about 13′ 41″ n. f. 4^m 20^s, and agreed in place with Σ 2547. The mean place of 37 *Aquilæ* deduced from the B. A. C. was for 1860·0, R.A. = 19^h 52^m $52^s{\cdot}66$, and N.P.D. = 100° 52′ 52″·5. In referring to each of these objects I shall designate them by Struve's numbers.

Σ 2541. Instantly seen *double;* looked carefully for a third star; none could be found. Magnitudes, A = 8½; B = 9½. Pos. = 340° ±; Dist. = 2″·5 ±.

Σ 2545. Instantly seen *triple*. Mag. A = 6½; B = 8; C = 11¼, *blue*. A B, Pos. = 315° ±; Dist. = 3″·0 ±. A C, Pos. = 170° ±; Dist. = 25″ ±.

Σ 2547. Obviously *triple*. Mag. A = 7½; B = 9; C = 10½. A B, Pos. = 330° ±; Dist. = 20″ ±. A C, Pos. = 140° ±; Dist. 45″ ±. *The three are nearly in a line.*

Of these three objects, I have not the slightest doubt that the *second*, Σ 2545, is H I. 13.

The only circumstance which presents any difficulty in the way of this conclusion is, that the distance of C from A is much greater than was estimated by Sir W. Herschel. But the marvel rather is that he *discovered* it at all with his 7-foot Newtonian; and a mere guess at the distance of a scarcely perceptible point of light may easily be much in error. A similar and very remarkable instance occurs in the distance of A C of Cycle 702, which was estimated by Smyth at 8″, though its real distance is between 40″ and 50″. It was, in fact, a mere guess, with *no weight* attached to it.

That the third star of H I. 13, should have escaped the scrutiny of such observers as South, Herschel II., Smyth, and especially Struve with his powerful Dorpat refractor; and yet should have been seen at all by Herschel I. with a reflector of only 6⅓ inches aperture (about equal in light to a refractor of 4½ inches aperture), and then, after lying hid for nearly eighty years, should have been caught up at the first glance with a telescope 1½ inch smaller in diameter than Struve's (and whose illuminating power is therefore only as 68 to 92) might seem almost with certainty to point to *variability of light*, as the only probable solution of the difficulty. Yet, during three years, since 1859, I have on various occasions examined it with a special view to this point, and have never found any proof of it, or even ground of suspicion. It is true that on a very clear night (19th August, 1862) I saw it with a refractor of only 4 inches aperture by Cooke of York, and with so low a power as 135; but, on viewing it with my 8¼-inch, I found

that all the faint stars in the neighbourhood were proportionably brighter than usual from the transparency of the atmosphere. The fact that Struve on three different nights overlooked the third star of Σ 2547, of which Smyth says, "It is plain enough in my refractor," and which has three or four times the light of the third star of Σ 2545 (H I. 13) is sufficient to show how easily such objects may be rendered invisible at a small altitude by a little unsuspected haze. Yet it would be highly desirable that this object should be put upon the list of "Suspected Variables." Of course, at such an altitude, great caution is required; and it will be necessary to compare it with two or three very faint objects in its immediate vicinity.

Hopefield Observatory, Haddenham,
October 1862.

Remarks on some Astronomical Eye-pieces.
By the Rev. W. R. Dawes.

As the improvement of astronomical eye-pieces must always be interesting to telescopic observers, it is hoped that a few remarks on that subject may not be unacceptable. They will have special reference to a paper by Mr. T. W. Burr, printed in the *Monthly Notices* for June 1862, in which he particularly recommends an eye-piece lately constructed by Mr. Thornthwaite, and, in consequence of its freedom from the defects of those in ordinary use, named the "Aplanatic" eye-piece.

It does not appear to be generally known that the late Carl Kellner, of Wetzlar, in Prussian Rhine, employed the same, or very nearly the same, form for his telescopes as for his microscopes—I have seen several of them. The field-lens in all was double convex; and so nearly equi-convex (in some, at least) that I could not discover that they were not exactly so. The eye-lens was a meniscus; and they all had a stop, or diaphragm, in the focus of the meniscus, and very near the field-lens. One of these, which produced a power of 80 on my telescope, had a field of 37′ 10″ in diameter; and, consequently, the angular subtense of the diaphragm = 49° 33′. Being directed to the full moon on the 8th of November, 1851, it embraced the whole of it with a considerable margin. On that night the moon's diameter was 30′ 6″; and, consequently, the breadth of the margin all round it was 3′ 32″. The focus being carefully adjusted on a well-defined object near the centre of the moon, the edge was very distinct all round without any perceptible colour.

In the year 1855, Professor Steinheil of Munich sent me three eye-pieces, the construction of which he considered an

improvement on Kellner's; the eye-lens being rendered a
matic by being composed of a double convex of crown-
and a plano-concave of flint-glass, cemented together.
seems to be precisely the same combination as that o
Thornthwaite's eye-lens, as described by Mr. Burr; and
it has already been in use for more than seven years.
many comparisons I have arrived at the conclusion, that
is little, if any, advantage in this construction over Kel
as made by himself.

Mr. Burr states that "the division of double sta
rendered easier by this [aplanatic] eye-piece. . . . Pe
who could not see ε *Lyræ* quadrupled with the Huygheni
123 had no difficulty in doing so with the aplanatic,
This is certainly very extraordinary. I have for many
been in the habit of showing this quadruple star, wi
refractor of only 2½ inches aperture, to persons who had
seen the object before; yet no one has ever experience
difficulty in perceiving both pairs to be double at the
time (without bringing each into the centre of the field)
the ordinary Huyghenian eye-pieces magnifying 93 and
times; and, as the two pairs are only about 3½ minutes
apart, this circumstance has never been supposed to in
peculiar excellence in the eye-pieces. I have lately tri
this object with the same telescope a Huyghenian eye-pie
Tulley producing a power of 120, with which it was impo
for any one to overlook the duplicity of both sets seen at
I presume that this condition (both pairs being seen doul
the same time, and of course in different situations in the
of view) is one particularly dwelt upon in the compar
because, if each is brought nearly into the centre, very
difference is produced in the image by the use of any
various kinds of eye-piece, whether merely a single le
composed of two. Under favourable atmospheric ci
stances I have examined α *Lyræ* near the meridian with a
every variety of eye-piece,—the Huyghenian, Kellner's
Steinheil's negatives, Ramsden's positive, or micrometer,
with ordinary and achromatic lenses; with double co
crossed, plano-convex, and achromatic lenses;—but whe
powers were nearly equal, I could perceive no difference i
definition of the disk, or in the formation of the optical
round it; the object being, of course, placed in the cent
the field, and kept there by the driving-clock. It is, how
obviously of the utmost importance in arriving at a c
result in such a comparison, that the lenses should all be
Any imperfection in their material or figure or workma
may be immediately detected by a distortion of the disk
bright star, or of the rings surrounding it; and the natu
the imperfection may probably be discovered by length
the focal adjustment considerably, when the defect will a
on the diffused image.

In reference to the size and distinctness of the field in compound eye-pieces, it is of course necessary, not only that the lenses should be good, but also that their focal lengths and distances from each other should be duly proportioned and adjusted. It is quite possible that any comparative inferiority in a compound eye-piece may arise, not so much from the character of its combination, as from some fault in its construction. For instance, — any Huyghenian eye-piece producing a power of about 125 on a good telescope of two and a half inches or more in aperture, which failed to show ε *Lyræ* quadruple, may on that very account be reasonably suspected of being imperfectly constructed, and therefore unfit to be employed as a standard of comparison for others.

I have lately had an opportunity of examining a Comet eye-piece made by Mr. T. Cooke of York on Kellner's plan, which fully comes up to the standard of those made by Kellner himself; the angular subtense of the diaphragm being nearly 50°, with remarkable distinctness and freedom from colour to the edge of the field.

In the "achromatic positives," as constructed by Steinheil, the eye-lens is a double achromatic; and there is an almost complete correction of colour to the edge of the field, which would be particularly advantageous in taking wide measures with the filar micrometer. In three which I have, however, the spherical aberration is over corrected, requiring the eye-piece to be a little withdrawn to bring the web into focus when far from the middle of the field. An eye-piece of this form, with large lenses and of low power, if correctly constructed so as to unite flatness of field with achromaticity, would make an excellent comet eye-piece; and it would be even more valuable for the annular micrometer, in which the colour infesting the objects and the edges of the rings in the micrometer, is a serious impediment to the accuracy of observation.

In the preceding remarks I have spoken of the *angular subtense* of the diaphragm of an eye-piece. This is a very useful *constant*, and may be easily determined by various processes.

1. Fix the eye-piece so that its diaphragm as seen with one eye can be projected on a wall, or a perpendicular staff, as seen with the other. Let an assistant mark the points on the wall cut by the upper and lower edges of the projected diaphragm, which will denote its apparent vertical diameter. Measure the distance between them, and also the distance of the wall from the eye, which should be opposite to the middle of the projected diaphragm. Then the angular subtense of the diaphragm will be found by the following formula:—

$$\text{Tangent } \tfrac{1}{2} \text{ angular subtense} = \frac{\tfrac{1}{2} \text{ projected diameter of diaphragm}}{\text{distance of the eye}}.$$

2. Another method of determining the same constant, and probably with greater accuracy, is by noting the time occupied by an equatoreal star near the meridian in traversing the diameter of the diaphragm of the eye-piece applied to a telescope. The magnifying power on that telescope being ascertained by a dynamometer or otherwise, the angular subtense is obtained by the formula,—

$$\text{Angular subtense} = \text{time noted} \times 15 \times \text{magnifying power.}$$

3. Where the eye-piece can be applied to an instrument having circles divided with sufficient minuteness, the diameter of the diaphragm in arc on that telescope can be obtained from the readings of the vertical circle, by bringing a star or any other well-defined object to the southern and northern extremities of the diameter, and taking the difference of the readings; then

$$\text{Angular subtense} = \text{angular diameter} \times \text{magnifying power.}$$

It is obvious that the angular subtense, or constant, of any eye-piece having been determined, the magnifying power produced by that eye-piece on any telescope may be readily found by merely observing the diameter of the field by any of the above-mentioned methods; when

$$\text{Magnifying power} = \frac{\text{angular subtense}}{\text{angular diameter}}.$$

Thus the diaphragm of an eye-piece whose *angular subtense* is 50°, if applied to one eye, and projected on the sky viewed with the other eye, would just include at the upper edge a star whose altitude is 50°, when the lower edge coincided with the true horizon. If such an eye-piece were applied to a telescope, and an equatoreal star were found to occupy two minutes in traversing the diameter of the field, the *angular diameter* on that telescope would be 30′, and the magnifying power 100.

As the focal images formed by an object-glass lie in a spherical surface of which the radius is the focal length, it is obvious that even if the spherical aberration of an eye-piece were perfectly corrected, it could not be free from the defect of indistinctness at the edge of a field of considerable extent. I am not aware that any successful attempt has been made to *over*-correct the spherical aberration of the Huyghenian eye-piece by making the *field*-lens a compound one; though there would seem to be no impossibility in the way of its accomplishment.

Hopefield Observatory, Haddenham,
Thame, September 1862.

On Aluminium Bronze as a Material for the Construction of Astronomical and other Philosophical Instruments. By Lieut.-Colonel A. Strange, F.R.A.S.

The author, after referring to the Astronomical and Geodesical instruments about to be constructed, under his superintendence, by order of the Government, for the use of the Great Trigonometrical Survey of India, and after noticing that one of the points which has given him most anxiety had been the selection of the proper material or materials of which to construct these instruments, and that his present remarks were confined to the great Theodolite with a horizontal circle 3 feet in diameter, and after detailing the requirements of such an instrument, proceeds as follows:—

"Such then was the problem presented for solution: to construct an instrument with extended powers, and cast as much as possible in masses, the transportation of which, over the most difficult ground, should not be beyond the power of human labour.

"When on the point of compromising the difficulty by separating, so as to form distinct packages, parts hitherto regarded as inseparable, my attention was attracted by the various articles made of 'Aluminium Bronze' sent to the International Exhibition by Messrs. Bell Brothers, of Newcastle, and M. Morin, of Paris. The inquiries I made in various quarters satisfied me that this metal possessed most valuable qualities, but I failed in my endeavours to obtain reliable numerical data for comparing it with other metals. I therefore instituted experiments on it, the results of which I beg now to communicate to the Society.

"The alloy called Aluminium Bronze was first, I believe, made by Dr. Percy five or six years ago, and is composed of Aluminium and Copper in various proportions, 10 per cent of Aluminium, however, giving the best material for mechanical purposes.

"The qualities of most importance in instrument making are, (1) Tensile strength; (2) Resistance to compression; (3) Malleability; (4) Transverse strength or rigidity; (5) Expansive ratio; (6) Founding qualities; (7) Behaviour under files, cutting tools, &c.; (8) Resistance to atmospheric influences; (9) Fitness to receive graduation; (10) Elasticity; (11) Fitness for being made into tubes; (12) Specific gravity.

"Of these, tensile strength, resistance to compression, and malleability, were most obligingly tested for me by Mr. Anderson at the Royal Gun Factory, Woolwich; and the other qualities by Messrs. Simms, by whom the great Theodolite is now being constructed. I will take the above enumerated properties in their order, premising that to have obtained results of an absolute and final character would have involved more time than I could spare, and that therefore those which follow, though sufficiently reliable for almost any practical

purpose, are open to the correction of more extensive experiments.

"(1) *Tensile strength.* This was tried some years ago by Mr. Anderson with the following results. Breaking strain

Aluminium bronze . . .	95,747	lbs. per square inch.
Gun metal	32,000	

"Mr. Anderson was good enough to try it again for me in September last, and states in his report, 'The average tenacity of this metal proved to be 22 tons 12 cwt. (50,624 lbs.) breaking weight per square inch in the two specimens tested; elongations did not take place until 4300 lbs. in the one case, and 3600 lbs. in the other, had been applied, when a permanent elongation was noticed of ·009 of an inch in the first specimen, and ·034 of an inch in the last.' Mr. Anderson adds that the specimens were not quite sound.

"In the above cited report Mr. Anderson gives a higher tensile strength to gun metal than before, namely, 17 tons (38,080 lbs.) for the average of the '*best specimens*' tested at the gun-factories.

"Combining the results we have the average breaking tensile strength of the two metals as follows:—

$$\left.\begin{array}{ll} \text{Aluminium bronze} & \dfrac{95747 + 50624}{2} = 73,185 \\ \text{Gun-metal} & \dfrac{32000 + 38080}{2} = 35,040 \end{array}\right\} \text{lbs. per square inch.}$$

the ratio being 1 to 0·48, or rather more than 2 to 1 in favour of aluminium bronze.

"For the purpose of comparing the tenacity of the new alloy with steel, we have data given by Mr. Anderson in a lecture* on Materials for Rifled Cannon, in which he states that cast steel varies in tensile strength from 114,000 to 67,000 lbs. per square inch; but he objects to the higher qualities as liable to brittleness, and prefers, where great strains are in question, an average quality of cast steel breaking at 80,000 lbs.; and he adds, that specimens from a gun made of Krupp's famous cast steel, characterised by 'softness' (which Mr. Anderson considers favourable to tenacity) and 'perfect soundness,' gave 72,000 lbs. per square inch, which we see is 1185 lbs. less than the average strength of aluminium bronze above given.

"(2) *Resistance to Compression.* Mr. Anderson reports on this as follows:—

"'The ultimate amount of compression applied was 59 tons, 2 cwt. 1 qr. 4 lbs. (132,416 lbs.), under which the specimen†

* Published in No. XXIII. *Journal*, Royal United Service Institution.

† "The specimen subjected to this enormous pressure, distorted though it is, does not exhibit the trace of a fissure. The cohesion of its particles is inviolate."

became too much distorted to permit of more weight being applied with any true result. Compression was not perceptible until 9 tons, 2 cwt. per square inch (20,384 lbs.) was applied, when the specimen suddenly gave to the extent of ·006 of an inch, and on the weight being removed an elasticity of ·001 was observed, which gives the first permanent compression as ·005 of an inch.'

"The compressive strength of cast iron varies a good deal. That of 'Carron Iron, No. 3,' the highest given in Ure's *Dictionary of the Arts*, is 115,542 lbs. per square inch; but it is difficult to compare in this respect two metals whose behaviour under compression is so different, cast iron yielding suddenly and almost totally, and the new alloy more gradually and partially. Astronomical instruments, however, are more dependent on the rigidity or resistance to a transverse force than to any other quality.

"(3) *Malleability.* Mr. Anderson states on this head:—

"'The qualities of this metal for forging purposes would appear to be excellent; with the exception of the part heated to a red heat in the shade, all show that it is a good workable material under the hammer almost up to melting point.'

"I may add that there were specimens in the Exhibition, showing that the alloy could be drawn out under the hammer almost to a needle point.

"I come now to the experiments tried by Messrs. Simms.

"(4) *Transverse strength.* As the absolute determination of the force necessary to break or permanently bend a bar of metal was beyond our appliances, I begged Messrs. Simms to be satisfied with a comparative value of the rigidity of the new alloy; that is, to ascertain the relative resistances of gun metal, brass, and aluminium bronze, to a force insufficient to produce permanent flexure. This they succeeded in doing, and report as follows:—

"'The same weight applied to three bars altered the index of our instrument as under,

Brass	2·22 divisions
Gun metal	0·15 ,,
Aluminium bronze	0·05 ,,

"Hence, aluminium bronze would appear to be 3 times more rigid than gun metal, and upwards of 44 times more rigid than brass.

"(5) *Expansive ratio.* This determination was also a comparative one. Messrs. Simms found that 'aluminium bronze is less affected by change of temperature than either gun metal or brass, (a little less than gun metal, and much less than brass).'

"(6) *Founding qualities.* Regarding this there is ample experience. The alloy produces admirable castings of any size.

"(7) *Behaviour under files, cutting tools, &c.* In this respect, also, it leaves nothing to be desired. It does not clog the file; and in the lathe and planing machine the tool removes long elastic shavings, leaving a fine bright, smooth surface. Messrs. Simms state,—

"'It can be worked with much less difficulty than steel, and we should think that screws made of it would (notwithstanding the original great cost of the metal) prove in the end less expensive than screws made of steel.'

"(8) *Resistance to atmospheric influences.* Messrs. Simms state, 'it does not readily tarnish.' This, likewise, is entirely a relative question. Absolute inoxidizability, however desirable, is hardly to be expected. Suffice it, that the new alloy tarnishes much less readily than any metal usually employed for astronomical instruments, viz., gun metal, brass, silver, cast-iron, or steel.

"(9) *Fitness to receive graduation.* Messrs. Simms state, 'Aluminium bronze takes a fine division, and it will not be necessary to inlay another metal, as is usually done, to receive the graduation.' This opinion is fully justified by the specimen of graduation executed by Messrs. Simms, the lines of which are remarkably pure and equable, characteristics never presented, in the same degree, they inform me, by lines cut on any other *cast* metal. May not this superiority indicate that the alloy in question is peculiarly homogeneous? The lines are very distinct under the microscope, notwithstanding the yellow colour of the metal.

"(10) *Elasticity.* I possess no direct experiments bearing on this point. But that the alloy has considerable elasticity is unquestionable. I may here state that an eminent Parisian Instrument-maker informed me that of all the wires tried for the suspension of Foucault's Pendulum for illustrating the rotation of the earth, none, not even those of steel, were so durable under that severe ordeal as wires made of aluminium bronze. It would appear, therefore, to be the most proper material for the suspension springs of clock pendulums.

"(11) *Fitness for being made into tubes.* It admits of every process necessary for this purpose. It can be soldered with either silver or brass solder; it can be rolled into sheet metal; and it can be hammered and drawn. Hitherto telescope tubes, the cones of transit axes, the pillars of altazimuths, &c., have been made almost exclusively of yellow brass, a metal very deficient in rigidity. Gun metal does not admit of being rolled, and has therefore never been used for the tubular parts of instruments, for which the new alloy seems pre-eminently suitable.

"(12) *Specific gravity.* The specific gravities of alloys of aluminium and copper, as determined by Messrs. Bell, are

3 per cent.	of aluminium	. . . . .	8·691
4 "	"	. . . . .	8·621
5 "	"	. . . . .	8·369
10 "	"	. . . . .	7·689

The last named, which is the best for the purposes now under notice, is very nearly the same as that of wrought iron, and less than that of either brass or gun metal.

"It appears from these experiments, and from the concurrent testimony of those who have given it a fair trial, that the 10 per cent Aluminium Bronze is far superior, not in one or some, but in every respect, to any metal hitherto used for the construction of Philosophical Apparatus, and that for such purposes it may be employed in the dimensions that would be proper in the case of cast steel. All parts which otherwise would be made of steel may, with perfect safety, and even with advantage, be made of the new alloy, particularly such parts as bolts, and fixing, tangent, and micrometer screws. Its hardness and comparative inoxidizability point it out as peculiarly adapted for pivots, axes, and bearings. If employed for receiving the graduation of circles, the necessity for inlaying another metal will be obviated, by which two advantages will be gained; the hammering which forms part of the operation of inlaying, and which, more or less, must cause unequal density and tension in the circle subjected to such treatment, will be dispensed with; and the effect of inequality of expansion in the circle and the inlaid strip will no longer be a cause of apprehension. With respect to the due visibility of divisions cut on this metal, opinions will perhaps differ. I can only say that I should be well content to observe with them.

"The use of this alloy for the construction of the new Great Theodolite is, in the opinion of Messrs. Simms, and in my own, fully justified by what we now know of it; and the effect of using it will be to keep the weight of the instrument within reasonable limits, notwithstanding its possession of means and appliances not hitherto bestowed on such instruments.

"Two points remain; the making of the alloy, and its cost.

"The metal Aluminium is at present extracted in England by one firm only, Messrs. Bell Brothers, Newcastle, under a license from M. Deville, the French discoverer of the process. I have met with instances of failure in making the alloy with Copper. Two main points only, however, seem to require particular attention. First, extremely pure copper must be used. The best is copper deposited by electricity, but that kind is very expensive; the next best is copper from Lake Superior, which makes an alloy of excellent quality. The ordinary coppers of commerce generally fail, owing, it is said,

chiefly to the presence of iron, which appears to be specially prejudicial. The second precaution is to re-melt the alloy two or three times. The first melting of 10 aluminium and 90 copper produces an alloy of excessive brittleness. Each successive melting, up to a certain point determined by the working, and particularly the forging, properties of the metal, improves its tenacity and strength. It is probable that after several meltings there will remain in combination with the copper a somewhat smaller proportion of aluminium than 10 per cent.

"The present price of the English made 10 per cent. Aluminium Bronze is 6*s.* 6*d.* per lb., but there is reason to believe that, as the process of extracting the aluminium becomes more largely practised, and the demand for the metal increases, the price will fall. The above price is four or five times that of gun metal. A much smaller quantity, however, of the new alloy than of gun metal will give the same strength; and when it is considered how small a ratio the cost of the material bears to the cost of workmanship in refined apparatus, it will be found that even at the present price of the new alloy its cost is not prohibitory, whilst the advantages attending its use promise to outweigh the increased expenditure.

"In the foregoing paper, which I am aware is very imperfect and incomplete, I have sought no more than to contribute such an instalment of practical information regarding this remarkable material as may be of service to those, who, like myself, contemplate making use of it, of whom I trust there will be many, for by its use we shall, I venture to think, confer on such structures at once greater strength and less weight, and so diminish tension, flexure, and distortion, to an extent calculated sensibly to improve the higher order of instruments of physical research.

On the Solar Energy as manifested in the Autumn of 1861 *and a Portion of the Spring of* 1862. By W. R. Birt, Esq.

During the summer of 1860, both previous to and in connexion with the great outburst of solar energy, that occurred towards the end of June in that year, I noticed a very marked monthly maximum and minimum of solar spots; and while there existed indications of an intermittent action of the solar force, I was satisfied that the maxima and minima I had frequently noticed were dependent on the rotation of the Sun.

Regarding each group of spots, single spots, if unattended by smaller ones, or separate nuclei if they were sufficiently distinct to warrant the appellation, as centres of energy, I laid

down from time to time curves of which the centres of energy were the ordinates.

Taking the solar meridian turned directly towards the Earth at noon of the 9th of July, 1861, equal to 270°, and employing the period of $27^d\ 7^h\ 10^m$ as that of the synodical rotation of the Sun, I set off 27 abscissa, each being distinguished by a Roman numeral I. to XXVII. preceded by the zero; and as the rotation of the Sun apparently carries a meridian about 13°·19 westwardly per diem, each ordinate may represent any solar meridian within the limits of 13°·19; thus the zero may represent any meridian between 353°·41 and 6°·59, and XIV. any one between 178°·02 and 191°·20, and so on.

I have the pleasure of submitting to the Society the curves of five synodical rotations from observations in August, September, and October, 1861, and April and May 1862.* Each presents a well-marked maximum, and the indications of minima are also apparent. The maxima are, without exception, acuminated, showing in each case not so much a diminution of energy as a removal from observation of the active solar surface by rotation.

The most instructive feature of the curves consists of the portions of the surface of the solar globe indicated as manifesting the greatest energy. 1861, August, the meridians 23° to 208° manifested the greatest energy during the period; indeed, a much greater activity than the opposite hemisphere. During the next rotation the energy was greatly subdued; the curve, nevertheless, exhibited *an increase* of force between 125° and 297°; and by projecting the September under the August curve, the similarity of features, as regards the increase and diminution of activity, is unmistakeable.

The October curve, after presenting us with a solar hemisphere perfectly quiescent, meridian 42° gives us increased action from 108° to 293°; indeed, there had been an increase of activity since 42° was central. Collecting these areas of increased energy, we have a portion of the solar surface extending over 274°, manifesting considerably more energy than the remaining portion of 86°. The following are the meridians of maxima:—August, 156°; September, 138°; October, 200°; and this extent of 62°, or thereabouts, may be regarded as the locality of greatest energy for those months, as referred to heliographic longitudes. The heliographic latitudes of solar energy are well known.

1862 the April curve exhibits subdued energy from meridians 14° to 173°, then increased energy to 332°, with a maximum at 292°. The May curve manifests during its earlier portion more activity than the April, with a minimum

* The drawings of these curves were exhibited at the Meeting in June last.—Ed.

at 156°. This was the meridian of maximum in August, so that the activity on the hemisphere, of which 156° is the central meridian, is now greatly reduced: in fact, the direction of the curve is reversed. From this meridian the curve manifests increased energy. The two curves for April and May presents as perfect an agreement in their features as those of August, September, and October, 1861. At the present time the hemisphere of greatest energy is marked by the central meridian 300°, or thereabout.

On a remarkable Chain of Lunar Craters forming a portion of the Southern Boundary of the Mare Serenitatis. By W. R. Birt, Esq.

The chain of Craters I am about to describe is but very seldom seen. I have only obtained three good views of it during the last eighteen months, viz., in 1861, April 16; 1862, May 5 and October 29. It is situated on the southern border of the Mare Serenitatis, between the conspicuous Crater Menelaus and a promontory running into the apparently open space forming the common boundary of the Maria Tranquillitatis and Serenitatis. This promontory is known as the Promontorium Acherusia.

The interest attaching to this chain of Craters will depend most probably on its having been overlooked both by Schröter, and Beer and Mädler. Although the Craters are small (they are larger than those in the neighbourhood of Eratosthenes) and situated on a narrow wall of rock running between the Maria Tranquillitatis and Serenitatis (if it be not situated on the surface of the Mare Tranquillitatis), the wall of rock rising nearly perpendicularly from level surfaces on the north and south of it, yet the locality is sufficiently remarkable, combined with the rarity of catching a sight of the Craters, to render it important to place a notice of their existence on record in the archives of this Society.

I first saw them in the year 1861, on April 16, my observations extending from 8 hours to 9·30, Greenwich Mean Time. The following is a quotation from my Journal of that date:—

"The Mare Serenitatis is separated from the Mare Tranquillitatis by a well-defined steppe, the Serenitatis being at the lower level.* From the Apennines, past Menelaus, to a point opposite Plinius, the border of the Mare Serenitatis is rugged, the highest portion of the wall from Menelaus to the terminating point consisting of a chain of small Craters—they are on the surface of the Mare Tranquillitatis, and appear at this epoch exceedingly interesting from their proximity to each other, the

* This steppe is admirably shown on one of Mr. De La Rue's photographs.

black shadows contrasting with the highly illuminated opposite rims. From the most western of these Craters, to Webb's Mount Argæus, the surface is very smooth, but the line of demarcation between the Maria Tranquillitatis and Serenitatis is well seen, and is exceedingly well defined towards its eastern extremity, near the small Craters. It is almost lost in the neighbourhood of Mount Argæus . . . . These Craters are exceedingly interesting, projecting, as they do, *alone* without any accompanying *ruggedness* into the smooth surface of the Mare Tranquillitatis."

On the 5th of May, 1862, I had an opportunity of minutely inspecting these Craters, and described in my Journal the wall in which they occur, from the Promontorium Acherusia to about one-third of the distance from the promontory to the bright Crater Menelaus, when the terminator skirted the eastern edge of the Mare Serenitatis. A quotation from my Journal will be the best description I can give:

"About two-thirds of the wall S.S.W. of the Mare Serenitatis, from the Promontorium Acherusia to the bright Crater Menelaus, is formed of small Craters interspersed with brighter portions hardly of the nature of peaks. The termination of the promontory is a bold, jutting, but low rock; it is succeeded by three deep Craters, hardly distinguishable from each other; they are quite in shadow. Two rocks and another Crater, which is isolated, form the further continuation of the wall. None of these Craters are given by Beer and Mädler. They are succeeded by a somewhat low portion of the wall to near the locality of the Crater B, of Beer and Mädler, where a jutting rock is seen. Adjoining and abutting on the Crater B, is another small, deep Crater, making the fifth from the termination of the promontory. The Crater B is nearly double the size of the small one on the border north of it, and it has a mountain on the south-west. Schröter has delineated it as being very much larger than Taquet, he marked it *g*. At a very little distance from the small Crater on the border, and separated from it by a low rock, are two more small Craters of the same character; they form the base of an isosceles triangle, Taquet being the apex. Another mass of rock intervenes, and we have the eighth Crater from the promontory. These Craters are pretty nearly of the same size, and differ but slightly from Taquet in this respect. The last two are the only Craters of the eight given by Beer and Mädler.

"In Beer and Mädler's map Taquet is given as larger than B. B is fully double the size of Taquet, and A on the south of the high land on which B is situated, and also on the border of the Mare Tranquillitatis, is about the same size as B. A is indicated as the largest on the map."

Schröter marks A (*e*), it is a little larger than Taquet in his delineation.

In connexion with these differences from Beer and Mädler's map, another Crater on the border of the Mare Serenitatis may be specified; it was observed the same evening, and is not indicated on the map. The record of it is as follows:—

"The depression *b*, Beer and Mädler, south-west of Menelaus, is finely marked; there is a Crater nearly north of it *on the wall*, it is larger than either of the previous eight, and is not in Beer and Mädler's map. This Crater is found in Schröter's figures marked *m*, also the shallow depression figured as a *deep* Crater marked *f*, the second instance of this kind which I have met with."

On the 29th of October I have the following notice: "The Craters on the ridge terminated by the Promontorium Acherusia very apparent, the terminating point *bright*."

The almost extreme difficulty of seeing these Craters, as they are visible but for a short time after sunrise—I have not seen them under the evening illumination—will, I apprehend, fully account for their having been omitted by the Selenographers above named. On a suspicion crossing the mind of the darker portions of the south wall of the Mare Serenitatis being the *shadows of cliffs*, I made the following note under date of May 5, 1862:

"The regularity appears to be too great to countenance this idea, the interior illumination is perceptible in that abutting on B of Beer and Mädler, also in the eighth, which is given both by Schröter, and Beer and Mädler, nor do the shadows fall quite in the direction for giving them the character of shadows of cliffs."

Some other communications received prior to the November Meeting, will be published in the next number of the *Monthly Notices*.—Ed.

Minor Planet (78) *Clytia.*

The Planet was discovered by Mr. Tuttle, at Harvard College, on April 7 (see *Ast. Nach.* Nos. 1368, 1370, 1374). Elements have been computed by Mr. Hall and Mr. Safford; those of Mr. Safford, from observations of April 7, 18, 25, and May 8 and 26, are

Epoch 1862, May $2^d\ 0^h$, Washington M.T.

L	=	184° 30′ 53″·6	Mean Equinox 1860·0
Π	=	61 11 13·5	
☊	=	7 32 23·7	
i	=	2 24 49·6	
φ	=	2 31 14·4	
μ	=	13 35·029	
log a	=	0·425889.	

Minor Planet (74) *Galatea.*

The discovery of this new Minor Planet by M. Tempel at Marseilles is announced, *Ast. Nach.* No. 1381, where the following positions are given:—

	h m	h m s	° ′ ″
Aug. 29	10	0 2 32	+3 55
30	10 30	0 2 12	+3 50 30

the planet being of the 11th magnitude. The following are the elements computed by Dr. Frischauf from observations of Sept. 16, 22, and 28.

Epoch, 1862, Sept. 16·0, Berlin M.T.

		° ′ ″	
M	=	353 48 52·9	Mean Equinox 1363·0
Π	=	6 42 36·5	
☊	=	200 29 20·2	
ι	=	3 15 43·8	
φ	=	14 12 26·3	
μ	=	858″·5920	
Log a	=	0·410813	

Minor Planet (41) *Daphne* ((76)?).

It appears (*Ast. Nach.* Nos. 1380 and 1382) that, at the Bilk Observatory, on the night of Aug. 31st, at about $11\frac{1}{2}^h$, Dr. Luther observed a planet of the 11th magnitude resembling the lost planet *Daphne;* and that, from this observation and two observations of Sept. 5 and Sept. 11, at Berlin, the following elements have been computed:—

Epoch, 1862, Sept. 5·5, Berlin M.T.

		° ′ ″	
M	=	88 51 17·5	Apparent Equinox at Epoch.
Π	=	233 44 39·8	
☊	=	179 7 7·7	
ι	=	14 38 49·4	
φ	=	16 47 27·7	
μ	=	728″·320	
Log a	=	0·458452	

which agree so nearly with Dr. Seeling's elements of *Daphne* as to leave no doubt of the identity of the two planets. The planet is however in subsequent numbers referred to as *Daphne* (76).

The name "*Melete*" instead of *Pseudo-Daphne*, given to the minor planet (56), has not hitherto been mentioned in the *Monthly Notices.*

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Dr. LEE, President, in the Chair.

Lieut. Allen Young, R.N., Riversdale, Twickenham, was balloted for and duly elected a Fellow of the Society.

A Last Letter from Capt. W. S. Jacob.

(*Communicated by Prof. C. Piazzi Smyth*).

"Having enjoyed the privilege," writes Prof. Smyth, "of receiving frequent letters from my estimable friend, Captain W. S. Jacob, during the whole of his previous career in India as an official astronomer, I hailed with exceeding delight, during last September, the arrival of a first letter, written on his once again returning to the land of his adoption and youth; now as a private astronomer, but with judgment matured, intellect strengthened, his own settled problems to work out, and a far more powerful astronomical instrument than he had ever had an opportunity of observing with before.

"But that first letter was destined also to be his last, for within a week of closing it, he was no more of *this* world. He had served the government previously, but too faithfully, first in a military capacity, and afterwards in the Madras Observatory; and had, as the result has proved, too nearly consumed for them his physical vital energies, while his mind was still active and dominant as ever.

"Capt. Jacob's letters, and his manner too, were never of a demonstrative character; but there was always a quiet worth about them, a calmness of view, sobriety of description, and easy introduction of numerical data, which rendered them of remarkable interest, and occasionally of extreme importance with reference to many scientific questions. These characteristics are all so well marked in his last letter, that I have thought for that reason, as well as because it is the last letter of a good and learned man, that the members of the Royal Astronomical Society might be interested in seeing it, recording it, and judging for themselves. It runs thus,—

"'*Ship Herefordshire, 25th July*, 1862.

"'My dear Smyth,

"'As we approach Bombay I begin a letter to have it ready to despatch on arrival, as I may not have time to write much then. We have had a tolerably pleasant voyage on the whole, the ship being well found and commanded, the party of passengers not too large, so as to incommode each other, and all having pulled pretty well together, so that there have been no quarrels, though a long sea-voyage seldom passes without them. Our freedom from them I attribute, in part, to the absence of *cards*, which often prove a fertile source of dispute.

"'I have been able to do but little in the way of work on the voyage; much less than I expected; there were perpetual interruptions, and though we have met with little or no very severe weather, there has been plenty of it sufficiently rough to prevent our settling to any quiet occupation, and especially the use of the microscope, with which I had hoped to do something. But in this I have quite failed. In the few attempts I have made, I did not succeed in finding any organism in the water at all. How to account for this is a puzzle to me; but I have also remarked that there has been throughout the voyage far less phosphorescence at night, than I remember on any former occasion; on many nights none whatever.

"'On the other leaf are a few memorandums which may interest you; you can make any use of them you please. The S.E. Trade has been irregular and squally on both sides of Africa: on *this* side it has occasionally blown for some hours from E.N.E., or thereabouts. So there have been anomalies in the atmosphere as well as the ocean.

"'*August 7th.* The same remarks will apply to the end of our voyage; when this was commenced we were expecting to reach Bombay in about a week, the passage from the Line being generally pretty rapid at this season, but instead of finding the S.W. monsoon at or near the Line, we met with light baffling winds from S., W., and even N.W., as far at lat. 10° N. For the last three days we have had the monsoon wind, but very little S. of W. We hope to anchor this after-

noon. All well, and in pretty good spirits, though we have been suffering latterly from heat more than I should have expected, the highest temperature having been 85°, but the light winds made it more oppressive. With our kind remembrances to ———,

"'Believe me,
"'Yours very truly,
"'W. S. Jacob.'

"11th August. Anchored on the 7th, at 2^h P.M. Leaving for Poona to-morrow morning. All well!

"*Zodiacal Light.*

May 17th, at 8^h P.M., in lat. 21° 1′ N. and long. 23° 20′ W.

Zod. light very bright, extending nearly to *Præsepe*.

	h	m		o	′
∴ apex in R.A.	8	31	and Decl. +	20	30
and Sun in R.A.	3	37	and Decl. +	19	25

May 26th, at 7^h 30^m P.M., in lat. 4° 45′ N. and long. 21° 20′ W.

Zod. light reaches nearly to *ι Leonis*.

	h	m		o	′
∴ apex in R.A.	9	35	and Decl. +	24	30
and Sun in R.A.	4	13	and Decl. +	21	12

June 11th, 16^h 0^m, in lat. 14° 44′ S. and long. 32° 15′ W.

Zod. light extends nearly to *Mars*.

	h	m		o	′
∴ apex in R.A.	23	13	and Decl. −	8	5
and Sun in R.A.	5	21	and Decl. +	23	9

June 16th, 7^h 0^m, in lat. 26° 30′ S. and long. 34° 0′ W.

Zod. light extends to a little beyond *Regulus*; axis points nearly to *α Virginis*.

	h	m		o	′
∴ apex in R.A.	10	5	and Decl. +	12	30
and Sun in R.A.	5	39	and Decl. +	23	22

June 21st, 7^h 0^m, in lat. 31° 53′ S. and long. 24° 55′ W.

Zod. light reaches nearly to *Saturn*, a little S.W.

	h	m		o	′
∴ apex in R.A.	11	16	and Decl. +	7	0
and Sun in R.A.	6	0	and Decl. +	23	27

July 16th, $6^h\ 40^m$, in lat. 29° 52′ S. and long. 55° 19′ E.

Zod. light seen along with twilight. At $7^h\ 0^m$ the vertex extends to half way between γ and α *Virginis*. The light is much paler than the brighter portion of the Milky Way.

	h	m		o	′
∴ apex in R.A.	12	57	and Decl. −	5	34
and Sun in R.A.	7	43	and Decl. +	21	20

July 17th, $7^h\ 0^m$, in lat. 28° 11′ S. and long. 57° 40′ E.

Sky hazy; the Zod. light is ill defined, but seems to reach a point about ⅓ of the way from *Spica* towards *δ Virginis*.

	h	m		o	′
∴ apex in R.A.	13	5	and Decl. −	7	15
and Sun in R.A.	7	47	and Decl. +	21	10

July 22d, $7^h\ 30^m$, in lat. 16° 2′ S. and long. 62° 23′ E.

Sky hazy; Zod. light very indistinct, extremity somewhere about *Spica*.

	h	m		o	′
∴ apex in R.A.	13	18	and Decl. −	10	26
and Sun in R.A.	8	7	and Decl. +	20	15

July 30th, $16^h\ 40^m$, in lat. 5° 5′ S. and long. 63° 20′ E.

Zod. light bright; extremity nearly two degrees north of *α Tauri*, noted while *Venus* was behind a thick cloud.

	h	m		o	′
∴ apex in R.A.	4	28	and Decl. +	18	15
and Sun in R.A.	8	39	and Decl. +	18	30

" *Other Phenomena.*

" May 21. In lat. 12° 20′ N., long. 25° 0′ W., *η Argus* is almost lost in the nebula—quite so to the naked eye. In binocular, it looks a little brighter than *ι Crucis*, but far below *δ*.

" June 11. Lunar eclipse: when totally immersed, the Moon was of a dull copper colour, with a silvery edge at the part last immersed.

" July 16. In lat. 29° 45′ S., and long. 55° 25′ E., a large meteor seen at $7^h\ 50^m$, about twice as bright as *Venus*, passing obliquely downwards to the left, a little below *Corona Borealis*. It disappeared and reappeared several times behind small clouds.

" July 17. *η Argus* is now scarcely so bright as *ι Crucis*.

" July 29. Many small meteors at about 15^h, most of them directed towards a point between *Polaris* and *Capella*, rather

nearer *Polaris!* One, brighter, crosses *Cygnus* from *Cassiopeia.*

"July 30. Many small meteors in various directions between 8^h and 9^h, and again about $16^h\ 30^m$."

Thus closes Capt. W. S. Jacob's last letter to me; wherein, the meteor of July 16th, with its notes of proof of a minimum distance; and the Zodiacal Light of July 30th, with its example of profiting by an accident, to eliminate a dangerous interference with the sort of observation he was engaged on, illustrate most truly the able observer and intelligent mind. I would also, and more particularly, beg to call attention to the extremely anomalous condition which the writer describes, as prevailing both in the sea and atmosphere; the latter evidenced in the perversion of both trade-winds and monsoons: the whole forming a most independent testimony to Sir John Herschel's opinion recently expressed, in the *Memoirs of the Manchester Philosophical Society;* to the effect, that the climate of the whole Earth has been experiencing, for the last two and three years, a remarkable disturbance from a cosmical cause, which he places in the Sun, and traces, first in its direct action on the Southern hemisphere, and then indirect action on the Northern.

In the section of the Zodiacal Light I have added to each observation, in parentheses, a note of the concluded Right Ascension and Declination of the apex of the light, as well as of the Sun, at the time, to facilitate deductions; and amongst the additional phenomena, have only to allude to the striking terms in which my deceased friend describes the decadence of η *Argus.* When I left the Southern hemisphere, in 1845, this star was brighter than any in the Northern hemisphere, and was the third brightest in the Southern, being transcended only by *Sirius* and *Canopus,* if, indeed, the latter was superior. It was then, without doubt, brighter than when Sir John Herschel left the Cape in 1838; but it had increased so much during his stay, that that great astronomer remarked, in a conversation there can now be hardly any blame in reporting, "that he was glad he had mapped down the nebula around η *Argus* during the earlier part of his visit, for that now (in 1837) the star was so bright as to have nearly extinguished the nebula," or words to that general effect. In the year 1862, however, behold Capt. Jacob beginning to describe η *Argus* as, in its turn, "almost lost in the nebula"—a remarkable inversion of the former particulars.

Observations of the Horizontal Refraction of the Sun.
By A. Lang, Esq.

The observations were made on the rising, from the horizon of the Sea, of the Sun's upper or lower limb, or both combined, so as to determine the exact moment of the rising of his centre, and from that the exactly computed zenith distance at that moment, for the purpose of obtaining the refraction, through comparison with the actual zenith distance of the horizon of the Sea, as measured and determined at the place of observation. The place of observation, "my Observatory" in the Island of St. Croix, in lat. 17° 44′ 32″ N., long. 64° 41′ W. from Greenwich; height of the axis of my Altitude and Azimuth circle (Troughton) above the level of the sea, 440 English feet, most carefully measured in every way (tube only a few inches); angle of depression of horizon of the Sea, as determined by the instruments, I have never found less than 20′ 52″, and never beyond 21′, and the last very rarely. I have therefore assumed the angle of depression for the following observations as 20′ 56″, or zenith distance 90° 20′ 56″; the height of my eye in observing the Sun being always at the same height above the Sea-level as the axis of the Circle, but in an adjoining apartment, near my mean-time clock, that I might hear and count the seconds. Telescope used, an excellent 44-inch refractor, using a magnifying power of 42, always previously fixed on the spot where the Sun would rise.

	Zenith Distance of Sun's Centre. By Computation.	Actual Observation.	Refraction and Parallax.	Barom. Ins. Pts.	Therm. Fahr.
	° ′ ″	° ′ ″	′ ″		°
1831, May 3	90 53 25	90 20 56	32 29	29·63	76
June 20	52 44·8	,,	31 48·8	·70	73
1832, June 5	52 58·7	,,	32 2·7	,,	79·5
Oct. 27*	54 4·5	,,	33 8·5	,,	,,
1833, July 31	53 11·5	,,	32 15·5	·63	79
Oct. 31	53 6·4	,,	32 10·4	·62	77·5
Nov. 1	52 50·5	,,	31 54·5	·53	78
1834, May 3	53 11·1	,,	32 15·1	·69	78
July 11	52 57·8	,,	32 1·8	·60	79
Oct. 25	53 6·9	,,	32 10·9	·62	79
1837, July 30	53 6·9	,,	32 10·9	·67	78
1839, May 17†	90 53 1	,,	32 5	29·635	77

* I am not satisfied with this observation.

† The observation of May 17, 1839, which I have carefully computed, should be more correctly stated 90°53′14″·82 (−90°20′56″ = 32′18″·82). It attracted my notice, as the time was exactly determined by a transit immediately afterwards. It is singular it should so exactly agree with the mean of 17 observations.

	Zenith Distance of the Sun's Centre. By Computation.	Actual Observation.	Refraction and Parallax.	Barom. Ins. Pts.	Therm. Fahr.
	° ′ ″	° ′ ″	′ ″		°
1840, May 11	90 53 47·7	90 20 56	32 51·7	29·64	77
1844, July 30	52 56·8	,,	32 0·8	·57	79
1846, May 13	53 42·4	,,	32 46·4	·61	79
17	53 37·6	,,	32 41·6	·55	78
1848, Feb. 17	90 53 22·7	,,	32 26·7	·55	70
	17)905 12·8		549 20·3	15)92·85	16)1240
	90 53 10·84	90 20·56	32 18·84	29·619	77·5
1857, Oct. 27*	90 53 25	,,	32 29	29·547	77·5
30	53 19	,,	32 23	·602	78
Nov. 1	53 22·6	,,	32 26·6	·593	78·6

The altitude of my Observatory above the level of the sea, 440 English feet, causes a depression in the barometer of 0·455 inch.

From my station, the rising of the Sun from the horizon of the sea can only be seen in May, June, July, part of October, November, and February. The setting of the Sun into the horizon of the sea, not at all. Moreover I have sometimes every morning, for six weeks running, watched, disappointed, to get an observation on his rising, in consequence of dense low clouds of cumuli at horizon. To this day I have not obtained a single observation this year. So it is not so easy a matter as may be imagined.

St. Croix, 11th July, 1862.

On the Practicability of observing the Occultation of Stars by the Moon at Sea. By David Smith, Esq.

The author, in his communication addressed to the President and Council of the Society, writes as follows:—

It is not without some degree of hesitation that I take the liberty of forwarding you the results of my own experience as regards the practicability of observing Occultations of Stars at sea. The desire I feel that this most invaluable method for determining the Longitude may come into more general use amongst Naval men will, I trust, be a sufficient apology for my addressing you.

The method, as far as I can ascertain, has been, but with few exceptions, wholly neglected at sea: this has, no doubt, arisen from a supposition that the motion of a vessel would

* "An admirable observation" is added to the observation in my Journal.

prevent using a telescope of sufficient power to observe the phenomena, as the stars most frequently occulted are small. Another reason may be, that a previous knowledge of the star or its relative position to the larger stars is requisite.

With respect to the objection on account of the vessel's motion, I have only to state that except when a ship is very uneasy and laboursome, that stars up to the third magnitude can easily be observed disappearing or reappearing at the Moon's dark limb; and that in smooth water, and the Moon not nearer than three days from the opposition, stars between the fourth and fifth magnitude can be observed with great precision.

As regards the power of the telescope employed, all that is necessary is a good ship's glass, such as are usually sold for 3*l.* or 4*l.* With instruments of this description I have observed ten occultations, which have yielded unexceptionable results. They stand as under:—

Disappearance of	* Regulus on the Moon's dark limb.		
,,	* *b* Ophiuchi	,,	,,
,,	* h^3 Sagittarii	,,	,,
,,	Saturn	,,	,,
,,	* φ Sagittarii	,,	,,
,,	* Antares on the Moon's bright limb.		

Reappearance of	* Antares on the Moon's dark limb.		
,,	* η Piscium	,,	,,
,,	* η Tauri	,,	,,
,,	* 27 Tauri	,,	,,

The two last-named stars were observed on the same night, and the results agreed to within two seconds of time.

In the case of the star h^2 *Sagittarii*, the star seemed to advance upon the Moon's disk before disappearance.

The disappearance of *Antares* on the Moon's bright limb gave a very satisfactory result, as was verified on our arrival at Colombo, Ceylon, some ten days after.

I may add that I have found Captain Shadwell's Tables (for finding the approximate times of beginning and ending) of great assistance, being concise and easy of application, and invaluable for finding the time of reappearance.

In the annexed papers I have given the calculation of the longitude by the Occultations of η *Piscium*, η *Tauri*, and 27 *Tauri*.

I may also state that I have observed the termination of two Solar Eclipses at sea. The resulting longitudes were very exact. One happened two days after leaving port, and the

other in sight of the Island of Basilan, near Mindanao; so that the chronometer's errors were well known.

By means of the Eclipse of the Sun and the Occultation of the star η *Piscium*, carried on by a very steady chronometer, I have during my last voyage been enabled to check the Longitudes of several most important points in the Eastern Archipelago.

Lavender Wharf, Rotherhithe, S. E.,
July 24th, 1862.

The following is one of the three calculated Occultations above referred to.

Occultation of ✱ η *Piscium by the Moon.*

On board the ship Marchioness, of Londonderry, July 1861, in the Java Sea; latitude 4° 49′·3 south; longitude by account 114° 55′ east. I observed the reappearance of the star η *Piscium* on the Moon's dark limb to take place on July 28th, $15^h\ 25^m\ 8^s$ apparent time at ship. A chronometer showed at the same time, $7^h\ 48^m\ 46^s$, its assumed error on Greenwich Mean Time being $2^m\ 50^s$ slow. Required the longitude of the ship, and the chronometer's error on Greenwich Mean Time by this observation.

		h m s
Time of emersion by chronometer, July 28th		7 48 46
Chronometer's assumed error, slow.....	=	+ 2 50
Greenwich date of observation	=	7 51 36

at which time we have the Sun's Right Ascension $= 8^h\ 31^m\ 59^s$, Moon's Declination 14° 21′ N., Moon's Equatoreal Horizontal Parallax = 54′ 16″·0, Moon's Semidiameter = 14′ 49″·2, the Star's Right Ascension $1^h 24^m 6^s{\cdot}17$, Star's Declination = 14° 37′ 58″·6 N., Reduction of Latitude — 1′ 55″, log. radius of the Earth at the place = 9·99999.

		h m s			° ′
Sun's right ascension.....	=	8 31 59			
Apparent time	=	15 25 8			
Right ascension of meridian	=	23 57 7	Latitude.....	=	4 49·3 S.
Star's right ascension	=	1 24 6	Reduct.	=	— 1·9
Star's hour-angle in time E.		= 1 26 59	Geocentric lat.	=	4 47·4 S.
h = ,, in arc	=	° ′ ″ 21 44 45			

☽'s equat. hor. par. log. 3·51268
Log. radius of Earth ... 9·99999

Log. reduced parallax	3·51267		3·51267		=3·51267
Latitude, cosine	9·99848	sine.....	8·92161	cosine ...	=9·99848
✱'s hour-angle h, sine	9·56876	✱'s δ, cosine	9·98568	✱'s δ, sine	=9·40248
				(h), cosine	=9·96846
p =	3·07991	K=263″·0=	2·41996		
☽'s declination, cos	9·98623			K′=−762″·2=	2·88209
	3·09368	K =	−263″·0		
Constant logarithm.....	7·92082	K′=	−762·2		
Cor. of ✱'s hour-ang. = −10′·34 =	1·01450	K+K′=	−1025·2	=cor. ✱'s decl.	=17′5″·2

✱'s hour-angle	=h=21° 44′·75	✱'s declination	=14 h 37 m 58 s·6 N.
Correction as above	− 10·34	K+K′−corr.	= − 17 5·2
✱'s hour-angle corr^{d}.	=(h)=21 34·41	✱'s decl. corr^{d}.	=14 20 53·4 N.

Computation of the first part of the Moon's Parallax in Right Ascension:—

Log p as in the preceding part	=3·07991
✱'s corrected declination cos	=9·98623
	3·09368
Constant logarithm	=8·82391

First part of ☽'s par. rt. ascen. log=1·91759=82^{s}·72 or −1^{m} 22^{s}·72

The ☽'s declination at the Greenwich date (corrected for 2^{s} difference) is 14° 20′ 58″·7 N.

Computation of the second part of the Moon's Parallax in Right Ascension:—

✱'s decl. corrected	14° 20′ 53·4″ N.	mean=β=14° 20′ 56″ N.
☽'s declination ...	14 20 58·7 N.	
Diff. =	5·3	✱ south of the ☽.
☽'s semidiameter=	14 49·2	
Sum =	14 54·5=894″·5	log.=2·95158
Diff. =	14 43·9=883·9	log.=2·94640
β	=14 20 56	cosec.^{2}=0·02753
		2) 5·92551
		2·96275
	Log 15	constant=1·17609

Second part of ☽'s par. right ascen.=+61^{s}·19 log 1·78666

		h m s			
✱'s right ascension		1 24 6·17			
Part 1st corr.	−	1 22·72	(minus because ☽ east of meridian).		
		1 22 43·45			
Second part	+	1 1·19	(plus because an emersion).		
☽'s right ascension	=	1 23 44·64			
			diff. 98·33	log.....	= 1·992686
,, at 7^h	=	1 22 6·31			
			diff. 114·26	log (ar. comp.)	7·942106
,, at 8^h	=	1 24 0·57			
			36″·00	constant log	3·556302
Correction in time	=	+ 3098^s·1			log = 3·491094
		+ 51^{m}38^s·1			
		h m s			
Greenwich time		7 0 0·0			
M.T. at Greenwich	=	7 51 38·1			h m s
					7 51 38·1
Equation of time	= −	6 10·1	Chronometer showed		7 48 46·0
App. time at Green.	=	7 45 28	Chron. slow, G.M.T.	=	2 52·1
App. time at ship	=	15 25 8			
Long. in time	=	7 39 40			
Longitude in arc		114° 55′ 0″ E.			

N.B. An error of 2 miles in the latitude does not affect the result, an error of 10 seconds in the hour-angle of the star or apparent time makes a difference of only 4^s·6.

The latitude had been deduced from the meridian altitude of ✱ *Schedar*, which happened soon after the occultation, and reduced to that time by the D.R. This was also checked from the preceding and following days' latitudes by the D.R. Allowing for the current, the two results agreed within half a mile.

The apparent time was deduced from four sets of observations of stars E. and W. of the meridian, taken with two different instruments, viz. two sets of ✱ *Aldebaran* E. and two sets by ✱ *Altair* W. These were again checked by observations of the Sun for time after sunrise, and agreed very closely.

Six hours after observing the occultation, we were on the meridian of the east end of the Island of Arentes, in the Java Sea. I then took sight of the Sun and Moon for time, the Sun being east and the Moon west of the meridian. The results showed the chronometer to be

	h m s	
	7 41 23·5	slow of the mean time of the place,
and the chron.-error being −	2 52·5	slow.
	7 38 31	

or the longitude of the east point of Arentes in arc = 114° 37′ 45″ E. of Greenwich.

This agrees nearly with Lieut. Raper's position: he gives the centre of the island $=114^\circ\ 37'$. The island extends about three miles E. and W.

Forty-seven lunar distances from the Sun, viz. 31 W. and 16 E., gave the chronometer an error of $2^m\ 51^s$ slow at the time of occultation, which would assign nearly the same value for the longitude of the island as given above.

The chronometer had a steady rate of $1^s{\cdot}6$, losing for about seventy days.

On the Variable Star R *Vulpeculæ.* By G. Knott, Esq.

From a comparison of my own observations with Mr. Pogson's useful Ephemeris, I had been led to suspect some irregularity in the period of this star, and in a short paper, which was printed in the last volume of the *Monthly Notices*, I communicated my suspicions to the Astronomical Society. The paper appears to have come under the notice of Dr. Winnecke of Pulkowa, who, by his observations and researches, may almost be said to have made the star his own, and in a letter to the Rev. R. Main (*Monthly Notices*, vol. xxii. No. 8), adverting to the subject he remarks that he was afraid that an error of transcription had crept into the elements he communicated to Mr. Pogson, and proceeds to give his definitive epoch and period founded on "seven maxima, observed in the course of three years, with reference to Piazzi's estimations of magnitude in August 1803," together with an Ephemeris for the years 1861, 1862. My own observations of two maxima present so fair an accordance with Dr Winnecke's predictions, that I venture to submit them to the Society, in the hope that they may possess some interest. My final results are as follow:

Observed maxima of R *Vulpeculæ*, 1861, December 30·5, mag. = 8·7. 1862, October 4, mag. = 8·2, being later than the calculated dates by 3½ and 4 days respectively, a difference which may *possibly* be due to *personal equation*. The period deduced from these observations, viz. 138·75 days, presents also a very satisfactory agreement with that given by Dr. Winnecke, and I think we may fairly infer that R *Vulpeculæ* belongs to the class of regular variables.

Woodcroft Observatory, Cuckfield,
November 17th, 1862.

Description of a New mode of obtaining the Value of the Divisions of a Transit Level. By Capt. W. Noble.

I have recently been occupied in re-determining the value of the divisions of my Transit Level, by a method which, while

it involves no new principle, is, I believe, in detail perfectly original.

The Level, the value of whose divisions was to be determined, was in the first place lashed securely to the declination circle of my Equatoreal. The micrometer having been screwed into the eye end of the telescope, the wires were separated by some known number of seconds of arc. The instrument being now firmly clamped in the meridian, the telescope was so adjusted that a star not very far from the equator should, during its transit, run along and be threaded by the *top* wire of the micrometer. Just before the star entered the field the level was carefully read. As the star approached the middle of the field the telescope was depressed by means of the tangent screw of the declination circle, until the star ran along the *bottom* wire of the micrometer, whereupon the level was again read immediately. The number of seconds of arc through which the telescope, declination circle, and level, had been moved, was of course that of the interval between the micrometer wires, and this, divided by the number of divisions passed over by the level bubble, gave the value of one division.

The possessor of a large meridian circle would doubtless find that the ordinary mode of lashing his level to this circle and of reading the arc passed over by means of the circle microscopes, would be more accurate and expeditious; but to those who like myself possess only an Equatoreal, I would commend my method as susceptible of very considerable precision.

I may, in conclusion, mention merely as a fact, without attempting to account for it, that I invariably obtained a larger value for the divisions of the level scale from daylight observations, than I did from those made at night.

Forest Lodge, Maresfield, Sussex,
November 12th, 1862.

Results of the Meridional Observations of Small Planets; Occultations of Stars by the Moon; and Occultation of Jupiter's Second Satellites; observed at the Royal Observatory, Greenwich, in October and November, 1862.

(*Communicated by the Astronomer Royal.*)

Themis (24).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Oct. 29	13 3 52·2	3 36 15·10	70 21 4·09
Nov. 27	10 46 17·8	3 12 38·23	71 40 47·37

Iris (7).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Oct. 3	9 57 43·1	22 47 5·02	86 16 41·77
9	9 31 39·9	22 44 36·92	86 54 20·02
10	9 27 26·6	22 44 19·41	87 0 17·11
11	9 23 14·9	22 44 3·59	87 6 8·46
14	9 10 53·1	22 43 29·46	87 22 56·42
16	9 2 49·2	22 43 17·35	87 33 26·04
18	8 54 54·2	22 43 14·12	87 43 17·15
24	8 32 1·9	22 43 57·37	88 8 53·89
29	8 13 58·5	22 45 33·81	88 24 59·68
31	8 7 0·2	22 46 27·44	88 30 1·89
Nov. 3	7 56 48·3	22 48 3·55	88 35 58·61
10	7 34 9·2	22 52 56·64	88 42 33·16
12	7 27 57·7	22 54 37·17	88 12 34·85
17	7 12 59·5	22 59 19·34	88 38 56·13

Pomona (32).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Oct. 3	11 29 51·4	0 19 28·51	82 54 34·44
18	10 19 41·1	0 8 14·99	84 39 36·75
20	10 10 33·4	0 6 58·84	84 52 46·23

Aglaia (47).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Oct. 3	11 55 31·7	0 45 12·98	83 57 44·08
24	10 16 19·6	0 28 32·23	85 4 20·05

Ausonia (68).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Oct. 14	8 20 7·1	21 52 35·07	101 28 58·68
16	8 12 37·8	21 52 57·65	101 22 50·13

All the observations of N.P.D. have been corrected for refraction and parallax.

Occultations of Stars by the Moon.

Day of Observation.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
1862.			h m s	
Oct. 11	κ' Tauri, disapp.	Bright	13 32 56·2	E.
11	κ' Tauri, reapp.	Dark	14 54 18·6	M.D.

The initials E. and M. D. are respectively those of Mr. Ellis and Mr. Dolman.

On Nov. 24, Mr. Roberts observed the Reappearance after Occultation of *Jupiter's* Second Satellite. The last contact took place at $18^h\ 27^m\ 20^s{\cdot}4$ G.M.T.

The *Ann Arbor Astronomical Notices*, No. 28, contains a paper by Truman Henry Safford, Assistant at the Observatory of Harvard College, on the Proper Motion of *Sirius* in Declination.

The author in the first place refers to a paper by Dr. C. A. F. Peters, in the *Astronomische Nachrichten*, Nos. 745, 746, 747, on the Proper Motion of *Sirius* in Right Ascension; in which, by first comparing the observed Right Ascensions of α and β *Orionis* in different years with the tabulated Right Ascensions in the *Tabulæ Regiomontanæ*, and thence inferring the corrections to be made to observed places (a process which amounts to the same as supposing that the tabulated Right Ascensions are correct, or subject only to constant error), and applying those corrections to the observed Right Ascensions of *Sirius*, and comparing the corrected observed Right Ascensions of *Sirius* with the tabulated Right Ascensions of *Sirius* in the *Tabulæ Regiomontanæ*, a series of irregularities in the Right Ascensions of *Sirius* are found, which are numerically represented by the following hypothesis. That *Sirius* revolves in an elliptic orbit, on the law of gravitation, round an invisible companion; the time of passing the lower apse being the year 1791·431, the mean yearly motion $7^\circ{\cdot}1865$ (or periodic time 50·01 years), and the excentricity 0·7994: so that, putting u for the excentric anomaly at any time, the mean anomaly or $7^\circ{\cdot}1865\ (t - 1791{\cdot}431)$ will be equal to $u - 0{\cdot}7994 \,.\, \sin u$ (from which equation u is to be found); and the correction to the tabular place of *Sirius* will be

$$0^s{\cdot}127 + 0^s{\cdot}00050 \,.\, (t - 1800) + 0^s{\cdot}171 \,.\, \sin (u + 77^\circ\, 41');$$

where $0^s{\cdot}127$ is a constant correction to the tabular Right Ascension, $0^s{\cdot}00050$ is a correction to the annual variation, and $0^s{\cdot}171$ and $77^\circ\ 44'$ are constants depending on the magnitude of the major axis, its position, the position of the line of nodes of the orbit on the celestial sphere, and the inclination of the orbit to the celestial sphere.

The author then proceeds to treat the irregularities in the Declination of *Sirius* in the same manner. He uses the observations of Bradley (omits those of Maskelyne and Pond before 1822, because the thermometer coefficient is erroneous and he has not re-reduced the observations), uses Bessel 1820, Pond 1822, Struve 1824, Argelander 1830, Taylor 1832·5, Henderson (Cape) 1833, Maclear (Cape) 1834, Airy (Cam-

bridge), Henderson (Edinburgh), Airy (12 year), Airy (6 year), Airy (later results), Busch (*Astr. Nach.* No. 422), Bond 1843, Moesta. Piazzi is omitted, as not sufficiently accurate. The author supports the exactitude of Bradley's result by comparison with Lacaille (1750) and Mayer (1756).

The stars of comparison employed are *α Virginis*, *α² Libræ*, *α Scorpii*, *α² Capricorni*. The tabular places are altered, when necessary, so as to introduce the constant of nutation 9″·22, instead of 8″·98 as in the *Tabulæ Regiomontanæ*, or 9″·65 as in the *Fundamenta Astronomiæ*.

The resulting irregularities in the declination of *Sirius*, now to be explained, are the following:—

	″		″		″
1755	−0·70	1833	−1·48	1843	+1·11
1820	−0·40	1834 (A)	−2·38	1844·5	+1·86
1822	−1·48	1834 (M)	−1·71	1850·8	+2·06
1824	−1·63	1837	−0·76	1855	+0·82
1830	−0·96	1838·5 (A)	−0·39	1856·5	+1·53
1832·5	−1·68	1838·5 (B)	−0·42		

No correction to mean places and mean annual variation destroys the periodicity of those terms, or even reduces the sum of squares of errors in a greater degree than from 33 to 25.

The author then proceeds to try the effect of introducing the same hypothesis as that found by M. Peters from the Right Ascensions; adopting the supposition of elliptic orbit, using the same time of passage of apse and the same excentricity, and determining the two constants still undetermined from comparison of the irregularities with the following formula:—

$$w + x\,(t - 1800) + y\,.\,\sin u + z\,.\,\cos u,$$

and finally obtains

$$0''\cdot56 + 0''\cdot0202\,(t - 1800) + 1''\cdot47\,.\,\sin u + 0''\cdot51\,.\,\cos u.$$

By introduction of this formula, the sum of squares of errors is reduced to one-eighth part of the least possible amount on the hypothesis of uniform motion.

It would seem, therefore, that great probability attaches to the hypothesis that, in the system of which *Sirius* is the only visible part, there is a large invisible mass present.

The greatest defect in the series of observations here treated is the lacuna between 1755 and 1820. A large part of this, perhaps, can never be supplied. The unfortunate introduction of Graham's quadrant destroys our confidence in the greater part of Bradley's results, and in Olufsen's corrections for the quadrant in subsequent years (which depend on Bradley). If, however, Bradley's later observations could be

plausibly corrected, the observations of Maskelyne, treated by the method of comparison-stars, would give good instrumental results. No uncertainty, such as is produced by wearing of centre-work of the quadrant, attaches to Piazzi's or Pond's observations.

The correction of the refractions would be no very great work.

On the whole, we could wish that the able author of this paper could be induced to give to it all the completeness which existing materials can enable him to give. G. B. A.

On the Forms of Lenses proper for the Negative Eye-pieces of Telescopes. By G. B. Airy, Esq., Astronomer Royal.

In the *Monthly Notices* for June and November last, there are discussions on the forms of lenses proper for the Negative Eye-piece. Perhaps I shall not do wrong in stating to the Society that as long ago as the year 1827 I made a most elaborate investigation of the properties of Eye-pieces as depending on the curvatures of their surfaces. The paper is entitled "On the Spherical Aberration of the Eye-pieces of Telescopes," and is printed in the *Transactions of the Cambridge Philosophical Society*, vol. iii. This paper had been preceded by one "On the Chromatic Aberration of the Eye-pieces of Telescopes," from which I had been able to infer the proportions of the focal lengths and intervals of the lenses, which (independently of their curvatures) destroy colour at the sides of the field, using but one kind of glass; and had selected some of these as examples to which the formulæ for Spherical Aberration were to be applied. One of these is the Huyghenian Eye-piece, with the following very common proportions:—Focal length of first lens or field-glass = 3 M; focal length of second lens or eye-glass = M; interval between the two lenses = 2 M.

The principal results as applying to the cases before us are the following:—

First, as regards Distortion.

I must refer to page 15 of the Memoir for the general formula; but I may quote the following special results:—

(*a*.) It is possible to destroy Distortion entirely, but not by the use of common forms (equi-convex or plano-convex).

(*b*.) The most favourable combination of common lenses is,—the first, equi-convex; the second, plano-convex, with its convex side toward the first lens, or with its plane side next the eye.

Second, as regards Indistinctness at the edge of the field.

From the general formulæ in page 35 it appears that it is

impossible in any eye-piece whatever, in which the lenses are all convex, to secure distinctness in the approach to the edge of the field, except, in some cases, by movement of the eye-piece; and the problem always is to diminish the Indistinctness as much as possible. For this purpose, in the Huyghenian eye-piece,

(*c.*) The different points of the image may be made distinct by a little sliding of the eye-piece, by a form very nearly the same as the following:

(*d.*) The best form is, for the field-glass, a meniscus, with convex side towards the object-glass, and radii as 4 : 11; and for the eye-glass a convex lens, the mere convex side towards the field-glass, and radii as 1 : 6. The constants by which the indistinctness in the two dimensions is expressed are $\frac{95}{126}$ and $\frac{61}{126}$.

(*e.*) The best combination of common lenses is, two plano-convex-lenses, the plane sides of both towards the eye. The constants of indistinctness then are $\frac{133}{126}$ and $\frac{175}{126}$.

(*f.*) For a single lens to produce the same power, the smallest values of constants of indistinctness would be $\frac{196}{126}$ and $\frac{476}{126}$.

The following rule, though not strictly accurate, will be found sufficiently accurate to give a very good practical determination of the curvatures of all eye-piece lenses in all cases. Trace the course of an excentric pencil through the eye-piece. Consider separately the convergences, &c. of the axis of the pencil with regard to the axis of the telescope, and that of the rays of the pencil with regard to the axis of the pencil. When both these convergences fall on one side (as in the Huyghenian field-glass) the lens ought to be meniscus. When they are at equal distances on opposite sides, the lens ought to be equi-convex. When the convergence of either is much nearer (the other being on the opposite side), the side of the lens next it ought to be plane.

1862, *December* 10.

At the November Meeting the Astronomer Royal gave an interesting *vivâ voce* account of some investigations which have been recently made to improve our knowledge on the Measures of the Earth and of the Heavens.*

The first part of the Astronomer Royal's Address was

* I am indebted for this article to J. N. Lockyer, Esq., Fellow of the Society, who has kindly undertaken to supply reports of *vivâ voce* communications at the Meetings. — Ed.

illustrated by a map of Europe, on which had been inserted, by hand, the principal lines of triangulation of the countries already surveyed, and also the positions of several towns in Russia which had been obtained after the manner of a ship at sea. A glance at this map rendered evident the fact that the triangulation from the Danube to the North Cape on the North, and from the middle of Russia to Valentia Island on the West, left little to be desired.

Referring to the first labours of the French in the last century, and those of the French, Russians, and Swedes in the present century for the measures of different *Arcs of Meridian*, the Astronomer Royal observed that Russia has taken the lead in the geodetical operations at present in question—the measurement of an *Arc of Parallel.*

This arc, extending from Valentia on the West to the town of Orsk, situated in the extreme East of European Russia, and in nearly the same latitude as Valentia, comprises nearly seventy degrees of longitude, and is, as remarked by the Astronomer Royal, perhaps the longest one that will ever be measured by man.

It was pointed out, by means of the map, that this project of the elder Struve's, which was warmly taken up by the different Governments, by no means required the measurement of the entire arc: the existing triangulation, extending (as before remarked) from Valentia to a point pretty well advanced, would only require linking together where gaps existed, and extending eastward to Orsk.

To derive the measure of the Earth from these operations, it was first necessary to find, by some lineal measure, the *length of this arc;* in other words, to find the *distance in yards* (our standard measurement) between Valentia and Orsk; and, secondly, *the difference of the local times* at the two stations.

First, as to the length of the arc. The British part of it required little attention, as (thanks to the admirable work of the Ordnance Surveyors and subsequent investigation) the actual distance from Valentia to Dover is certainly known to within a few yards. Unfortunately the distance from Dover to Paris (equally well known) will not serve us, Paris being too far south. It became necessary, therefore, to effect a junction with the continental triangulation at some more northerly point; and this has been admirably effected, during the present year, by Sir H. James.

The whole distance from Valentia to some station well advanced is by this time computed, while the triangulation itself is fast approaching Orsk.

Secondly, as to the differences of local time at Valentia and Orsk. This, in an arc of such magnitude, can only be obtained from the differences of local times at intermediate stations in the arc; as, for instance, between Valentia and

Greenwich, Greenwich and Brussels, &c.; and in these determinations, as suggested by the illustrious projector of the operations, railways and telegraphs must be freely taken advantage of.

The difference of local times at Valentia and Greenwich was, as is well known, the subject of elaborate inquiry, some years ago, when several transmissions of chronometers were undertaken between the two places. The results obtained on this occasion have recently been confirmed in a remarkable manner, a complete electric circuit between the two places having been placed at the disposal of the Astronomer Royal by Sir Charles Bright. The difference of times obtained by the electric current (which, it may be stated *en passant*, took one-tenth of a second to traverse the 800 miles of wire) *agreed exactly* with that obtained from the chronometric expedition.

The second part of the Address related to the dimensions of the Solar System.

The recent researches into the velocity of light by means of the turning mirror—an instrument which, as used by M. Foucault, renders this velocity, inconceivable though it be, measurable by a cabinet experiment—and the parallax observations of *Mars*, which have been made during the autumn of this year in both hemispheres, were alluded to by the Astronomer Royal as affecting the determination of the Sun's distance, the unit of all measures of the Heavens.

The smaller velocity of light announced by M. Foucault was shown to necessitate a decrement of the Sun's distance, the two being connected by aberration; for as the umbrella of him who is caught in a shower inclines to meet the rain at a certain angle which depends upon the relative velocity of the rain-drops and the rate at which he is walking, so does the telescope point in advance of the star at a certain angle dependent upon the relative velocities of the light which enters it and the Earth which carries it.

This angle shows that the Earth travels (speaking in round numbers) 10,000 times more slowly than light. If, therefore, it is established that the received velocity of light must be diminished, the diameter of the Earth's orbit, which satisfies the received velocity, must be decreased in the same proportion.

The Sun's parallax, $8''\cdot5776$, deduced from the transits of *Venus* in 1761 and 1769, has also been called in question by M. Le Verrier, on other considerations; and the extreme doubtfulness of some of the most important observations of the transit of 1769 renders the success of those of the planet *Mars* doubly desirable; and although, by the latter method, the Sun's distance is only *indirectly* obtained (that of *Mars* resulting *directly* from the observations), the distances are bound together by a proportion which, as remarked by the

Astronomer Royal, was accurately known in the times of Copernicus and Tycho as at present.

There is reason to hope that the attempt of the present year will be attended with success. A fair number of eastern and western observations for the independent determinations have been obtained at Greenwich, and more are expected from Madras and Williamstown; and the meridian observations of Polar Distance (which require the co-operation of northern and southern observers) have been undertaken with vigour at all the Observatories of both hemispheres.

The Astronomer Royal also exhibited an original drawing of the fringes of light formed on a wall immediately before the disappearance of the Sun in the Total Eclipse of 1861, Dec. 31, prepared by a French officer, M. Poulain, Capitaine du Génie, at Goree.

In the accounts of the Eclipse of 1842 collected by M. Arago, and in those of the observers of later eclipses, it has been stated that in the last seconds before total obscuration there is a strange quivering of light upon the ground, &c., like the flitting of small shadows. The Astronomer Royal, in returning from his observing-station in Spain after the Total Eclipse of 1860, July 18, was accompanied in one journey by our Medallist, M. Goldschmidt (an excellent linguist), and by a Spanish gentleman who had observed the eclipse without instruments. This gentleman had been much struck by the sight of the flitting shadows; but, in the conversation which the Astronomer Royal held with him through the interpretation of M. Goldschmidt, it did not appear that the observations were made with such accuracy as to give useful information on the phenomenon.

On the occasion of the Total Eclipse of 1861, Dec. 31, M. Poulain, Capitaine du Génie in the French army, found himself at Goree, with the charge of observing the eclipse if possible; but without telescope or other instruments of the slightest accuracy. With great judgment, he applied himself to make the best use in his power of the means at hand. Meteorological observations of several classes were made with care, and recorded in a most orderly form; on these, however, the Astronomer Royal did not then insist, but proceeded to the delineation of the fringes, which M. Poulain had very carefully observed. The Sun was shining on a vertical white wall directed from east to west, and on this wall, a very short time before the totality (*un instant avant l'occultation*), the fringes were seen; and they were subsequently drawn by M. Poulain in their exact size (*en vrai grandeur*) on the sheet then exhibited. The only account of them given in the detailed record

is, "On voit des franges noires se dessiner sur le mur qui borde notre terrasse." The drawing exhibits five bands; three white, and two dusky-dark, each 4 inches broad; and their form seems to imply that in the longitudinal direction their length was considerable, and that in the lateral direction the repetition was more frequent. Their inclination to the vertical is about 45°; the right-hand end, referred to the hand of a person who is looking at the wall with his back to the Sun, being the lower.

At the Astronomer Royal's request, Mr. Hind was so good as to calculate the position of that point of the Sun's disk which was the last to disappear. It was at 31° from the Sun's vertex towards the east, or towards the left hand of an observer whose face was turned to the Sun. On comparing this with the position of the fringes, as depicted by M. Poulain, and referring the latter roughly to the direction of the Sun's rays, it appears that we may state, with as great accuracy as the observation permits, that the length of the fringes was in the same plane as the tangent to the disks of the Sun and Moon at the place of last disappearance.

The first impression from these appearances is, that they are diffraction-fringes; but the possibility of this explanation would seem to be destroyed by the consideration that any diffraction-fringes must travel with a linear velocity equal to the Moon's linear velocity, and must therefore be totally invisible. The phenomenon is one which deserves the careful attention of observers and optical philosophers. Meantime we may congratulate ourselves that we have for the first time a representation of this singular appearance aspiring to the character of reasonable accuracy. G. B. A.

Extract of a Letter from Herr A. Auwers to the Astronomer Royal, dated 5, Gartenstrasse, Gotha, 1861, *December* 1. —(Translation.)

Perhaps it is not uninteresting to you to know the result of researches on Parallaxes [of Stars] which I have carried out during the last two years at Königsberg. It is known that Struve, from observations with the Pulkowa Refractor for the parallel of 61 *Cygni*, has obtained a much larger value than Bessel had found with the Königsberg Heliometer, namely, $0''\cdot5107 \pm 0''\cdot0282$, instead of $0''\cdot366 \pm 0''\cdot012$. For that reason, I have effected a new determination with the Heliometer, in which I have made use of two comparison-stars different from those used by Bessel, which permit the application of the principle of differences in the most direct manner possible.

From observations on 62 days between 1860 September and 1862 June, I have obtained

$$\text{Parallax of 61 Cygni} = + 0''\cdot5656 \pm 0''\cdot0158.$$

The difference of this determination from Struve's is scarcely greater than its mean error. The mean from both is,

$$\text{Parallax of 61 Cygni} = + 0''\cdot5526 \pm 0''\cdot0138;$$

greater than Bessel's determination by $0''\cdot19$.

Another star whose parallax I have determined is the star with large proper motion lately discovered by Prof. Argelander, namely, Lalande 21258. This star (of the 8.9 magnitude) I have compared on 64 days, from 1860 November to 1862 June, by means of the Heliometer, with one star (of the 8 magnitude) preceding it by $4^m\ 11^s$ and with one star (of the 9 magnitude) following it by $3^m\ 56^s$; and from the differences of observed distances I have found

$$\text{Parallax of Lalande 21258} = + 0''\cdot2622 \pm 0''\cdot0109,$$

relative to the mean of the two comparison-stars; or absolutely, in conformity with the determinations of Professor Peters,

$$\text{Parallax of Lalande 21258} = + 0''\cdot3709 \pm 0''\cdot0112.$$

[*Note by the Astronomer Royal.*—The proportion of the discordance of the determinations of the same element, by different observers, to the magnitude of the probable error which each has assigned, throws great doubt upon the validity of such determinations of probable error.]

In a letter, dated Dublin, Nov. 12, 1862, Mr. Stothard notices the extraordinary brightness of *Aristarchus*, seen by him as a bright speck on the Moon's disk, that morning about $10^h\ 30^m$ A.M. Mean Dublin Time, in bright sunlight.

Mr. Grove, in a letter, dated Upper Harley Street, 5 Nov. 1862, gives an account (illustrated by three pencil sketches) of the appearances presented by the planet *Mars* on Oct. 26 and 31, and Nov. 3: the telescope used was $4\frac{1}{2}$ inches aperture; focal length 6 feet 2 inches; object-glass by Cooke, of York, capable of dividing γ *Coronæ*. He is perfectly satisfied of there being notable changes in the distribution of the lights and shadows, inconsistent, it appears to him, their being land and water, or, as he should perhaps say, land and water *only:*

clouds condensed over large aqueous districts might possibly account for the changes observed; the changes of position by axial revolution would not explain them.

A paper has been received from Prof. A. D. Wackerbarth, "On the Motion of a Pendulum with especial reference to the Foucaultian Experiments." The greater part of the paper is occupied in the investigation of the equations of motion, which are carefully and elegantly worked out; but the integration of them is developed only so far as to give the ordinary approximate result of a rotation of the plane of oscillation, with a velocity proportional to the sine of the latitude.

A paper by Mr. Wray, "Observations on *Saturn*," will be published in the following Number of the *Monthly Notices*.—Ed.

In a letter dated Hamilton College Observatory, Clinton, N.Y., 1862, Nov. 24, addressed to the Astronomer Royal, Dr. C. H. F. Peters gives, for Minor Planet ⓝ *Feronia*, the following elements, which are slightly different from those communicated to the *Ast. Nach.* (under the date Nov. 12, see No. 1396):—

Epoch 1862, Dec. 31·0 Greenwich M.T.

		°	′	″	
M_0	=	73	4	56·48	
π	=	309	48	37·93	Mean Eq. 1863·0.
☊	=	207	37	13·11	Mean Eq. 1863·0.
i	=	5	25	55·22	Mean Eq. 1863·0.
φ	=	6	41	17·06	
μ	=	1034″·0617			
Log μ	=	3·0145464			
Log a	=	0·3569735			

(The planet 12-11th magnitude.)

And he gives notice of another new asteroid, which he saw first 12th November and last 23d November; on both which evenings, however, it was clear only for so short a time that he could not determine the position accurately. A good observation (the only one then obtained) was

	H. C. Mean Time.	R.A.	Decl.	
Nov. 15·	$7^h\,26^m\,30^s·3$	$0^h\,2^m\,8^s·93$	$+1°\,27'\,12''·2$	(6 comp.)

The magnitude of the planet was about 11·7. The places on the other days mentioned differed from that of the 15th about thus:—

Nov. 12	0·6 less in R.A.	1′ 49″ further South.
„ 23	19 more in R.A.	21 40 further North.

these motions being taken only by measurement from the chart.

Comets III. and IV. 1862.

(*Letter from Dr. Bruhns.*)

"*Leipzig*, 6 *Décembre*, 1862.

"Je me permets de vous envoyer des éléments et des éphémérides des deux comètes qui peuvent servir à faciliter leur observation. Les éléments de la première comète sont calculés par M. Engelmann au moyen des observations des 1, 2, et 3 Décembre, ceux de la 2e comète par moi-même au moyen des observations des 1, 2, et 4 Décembre.

	Eléments de la 1e Comète (Mouvement rétrograde).	Eléments de la 2e Comète (Mouvement rétrograde).	
	Temps de Berl.	Temps de Berl.	
Passage au Périhélie	1863, Fév. 1·708	1862, Déc. 31·1959	
	° ′	° ′ ″	
Longitude du Périhélie	34 18·5	128 12 55·3	Equinoxe 1863·0.
„ du Noeud	114 31·0	358 16 11·5	
Inclinaison ...	86 59·5	48 22 23·3	
Log. de la Dist. Périhélie	9·85124	9·935674	

Ephéméride de la 1e *Comète pour* 0h *Temps Moyen de Berlin.*

	R.A.	Decl.	Log Δ.	Eclat.
	° ′	° ′		
1862, Nov. 30	157 39·8	− 3 19·2	0·0124	1
Déc. 4	160 39·0	− 1 33·0	9·9601	„
8	164 16·9	+ 0 48·1	9·9021	1·7
12	168 54·8	+ 4 1·0	9·8378	„
16	175 4·6	+ 8 26·8	9·7676	3·1
20	182 30·7	+14 32·9	9·6945	„
24	196 14·7	+22 27·1	9·6296	5·8
28	214 15·3	+31 0·0	9·5912	„
1863, Jan. 1	236 48·9	+36 56·9	9·5980	6·7
28	305 30·6	+23 51·2	9·9995	1·1

Ephéméride de la 2e Comète pour 0h Temps Moyen de Berlin.

	R.A.	Decl.	Log Δ.	Eclat.
	° ′	° ′		
1862, Nov. 30	208 59·9	−12 56·4	0·2153	1
Déc. 4	209 13·6	−15 39·7	0·1853	"
8	209 29·7	−18 48·4	0·1520	1·5
12	209 50·1	−22 58·4	0·1068	"
16	210 18·1	−26 55·5	0·0750	2·5
20	210 58·8	−32 17·7	0·0314	"
24	212 2·5	−38 54·3	9·9854	4·0
28	213 49·8	−47 3·8	9·9394	"
1863, Jan. 1	217 13·0	−57 0·7	9·8975	6·2
28	15 31·6	−45 25·6	0·0043	2·8
Fév. 17	20 7·2	−24 40·8	0·2091	0·8

"Tandis que la première comète sera visible dans nos observatoires boréals, l'autre disparait déja au milieu de Décembre par suite de sa déclinaison australe, mais elle va reparaître probablement au milieu de Février."

A few Notes on Prof. C. P. Smyth's Experiences with the Elchies Refractor. By the Rev. W. R. Dawes.

In the interesting account given by Professor C. Piazzi Smyth in the *Monthly Notices* for November last of his "Experiences with the Elchies equatoreally mounted Refractor of 11 inches aperture," he refers to a group of stars, of which P. i. 222, is the brightest, as having very surprisingly varied in their magnitudes since they were measured by his Father in 1834, in whose "Cycle" they form No. 78. This led me to give them an early examination; and I was astonished to find that no perceptible variation had taken place. On comparing the object presented to my view in my 8¼-inch refractor, with a power of only 94, with the Professor's account of it and with Admiral Smyth's measures, I immediately perceived that the Elchies Refractor had not shown the star B at all!—which is the more surprising as it was obvious enough in my telescope at the first glance with so low a power. Moreover, in the "Cycle" there is a transposition of the magnitudes and colours of A and D,—the latter being P. i. 222, a star rather below the 6th magnitude. This mistake, combined with the absence of B in his telescope, has led the Professor to surmise a total change in the group. The three stars to the south of P. i. 222, form the triple star Σ 196; and the fol-

lowing comparison of magnitudes, position, and distances will show that all the components are *in statu quo*:—

Σ 196; $\alpha = 1^h\ 51^m\ 59^s{\cdot}5$; $\delta = +20°\ 20'\ 32''$; Epoch 1863·0.

	Magnitudes.					Positions.				Distances.			
	Σ	Sm.	*Sm.*	D.		Σ.	Sm.	*Sm.*	D.	Σ.	Sm.	*Sm.*	D.
A	8½	9	12	9									
B	11	15	not seen	10½	AB	55·53°	53·0°	...	53·7°	2·37″	2·5″	...	2¼″ ±
C	9¼	10	12	9¼	AC	167·38	165·0	169·25	165·6	39·46	40·0	36·94	36·2 ±
D	...	6	7½	6¼	AD	...	359·2	360·35	361·6	...	165·0	880·54	182·5 ±

My positions and distances were only single measures with a very low power, for the sake of certain identification. Struve has not included the bright star D, which is, in fact, too distant to belong properly to the object he measured. How it should have happened that, though the group was so carefully examined with the Elchies Refractor, that fine instrument failed to show the star B, remains to be explained; for it certainly ought to be obvious enough in such a glass. It is an important fact, however, as showing how unsafe it is, because an object was not seen when it might have been supposed that it could not possibly have escaped observation, to conclude from this that it must therefore be variable in brightness.

With reference to the visibility of such very minute stars as are many of those which are adduced in proof of the optical power of the Elchies Refractor, it would, I think, be a great mistake to assume as certain, or even as very probable, that, because the existence of such minute specks, at a distance of one or two minutes, is not recorded, *therefore* they were not seen; and it would be a still greater mistake to assume that they *could not* have been seen, or even micrometrically measured, with the instruments in use, if the design of the observers had been to test the optical powers of their telescopes. For instance: as the group round P. xxi. 312, contains some of the smallest stars quoted as discovered by the Elchies Refractor, I turned my telescope upon it; and though the night was rather hazy and the star nearly four hours from the meridian, all the stars seen at Elchies were brought out without the slightest difficulty with power 94, except C, which, being pretty near the brightest star, required a higher power to draw it out.

The most valuable property in a telescope is undoubtedly a *fine sharp definition;* and wherever this exists, even in an ordinary degree, a very simple calculation will show of what magnitude a star must be to be visible with any given aperture. Thus an object-glass of 11 inches in diameter will show a star whose magnitude is about 12¼ of Struve's scale, or 18½ of Sir John Herschel's. Nor is the visibility of such a star, when *isolated*, greatly affected by the quality of the object-glass, unless very bad indeed; but it is far otherwise when a very

small star is within 2″ of one much brighter than itself; for then the visibility of the small star will chiefly depend on the fine definition of the brighter.

As there are now so many large telescopes, both refractors and reflectors, in existence and in process of manufacture, I hope soon to present to the Society a list of objects most suitable for severely testing their powers of definition, and well situated for observation in this latitude.

Hopefield Observatory, Haddenham, near Thame,
Dec. 10, 1862.

RECENT PUBLICATIONS.

The Earth and its Mechanism; being an Account of the various Proofs of the Rotation of the Earth: with a Description of the Instruments used in the Experimental Demonstrations. To which is added, the Theory of Foucault's Pendulum and Gyroscope. By H. Worms, Esq., F.R.A.S., F.G.S. London, 1862.

The work is intended as a complete collection of the various demonstrations of the Earth's Rotation, and the theorems connected therewith; the principal object being to promulgate a knowledge of the more recent discoveries and experiments relating to this branch of science. Thus particular attention is drawn to Arago's application of Wheatstone's revolving mirror for the measurement of the velocity of light. A minute account is given of the latest experiments on falling bodies, made by Prof. Reich in the mines at Freiberg, in addition to those of Benzenberg and Guglielmini; the phenomena exhibited by Foucault's pendulum and gyroscope are carefully explained, &c. The second part, intended for the perusal of the mathematical student, gives, from Hansen's *Theorie der Pendelbewegung* (Danzig, 1853), with some omissions and additions, an investigation of this dynamical problem of the motion of a pendulum as affected by the rotation of the Earth.

Annals of the Astronomical Observatory of Harvard College, Vol. III., Cambridge (U. S.), 1862.

The volume has the second title, "Account of the Great Comet of 1858, by G. P. Bond, Director of the Observatory of Harvard College."

The following list of the titles of the several sections will give an idea of the contents of this splendid Monograph of Donati's Comet:—

I. Figure and Position of the Tail.—Comprising the original notes of observation upon the principal tail, between August 14th, 1858, and February 9th, 1859, quoted with full references to the original authorities, and accompanied by remarks indicating the principal points of interest. This section is illustrated by twenty-three engravings, on steel, of the appearance of the Comet to the naked eye, and by eight wood-cuts.

II. Observations upon the Secondary Tails.—The text is illustrated by three wood-cuts, and by the engravings and charts of the naked-eye views.

III. Reduction of Observations upon the Figure and Position of the Tail.—The results of the discussion are exhibited on a series of charts giving the normal outlines of the principal tail on twenty-eight dates, between September 16th and October 17th inclusive, and the axis of the principal secondary tail for seventeen dates.

IV. Probable Errors of Observations upon the Tail.

V. On the Deflexion of the Tail.—Describing a peculiarity in its upper regions during the first and part of the second week in October. Two plates of outlines and a wood-cut accompany the text.

VI. Columnar Structure of the Tail.—Relating to a remarkable feature faintly exhibited in the upper part of the tail.

VII. Reduction of Observations on the Secondary Tails.—The results of this discussion have been entered on the charts, with the outlines of the principal tail.

VIII. The Nucleus and Envelopes.—The original accounts descriptive of the telescopic appearance of the Comet are here quoted in the same form as in Section I. They are accompanied by twenty steel engravings.

IX. On the Outline of the Head of the Comet.—The discussion is illustrated by two plates, showing the normal outlines of the head of the Comet between Sept. 3d and Oct. 18th.

X. On the Branches and Central Darkness of the Tail.—Engravings of nine views of the Comet, 1860, III. are given in this Section.

XI. The Nucleus.—Containing a description of the nucleus, with particular reference to its size and changes of brightness.

XII. The Envelopes.—The various phenomena of the envelopes, their velocity of ascent, the dark and bright spots on their surface, and the question of their rotation or oscillation, are considered in this Section, which is illustrated by a lithographic plate of 138 telescopic views. The Section concludes with a notice of the envelope formation in the

Comet, 1860, III. and in the great Comet, 1861, II. accompanied by a steel engraving of the former.

XIII. The Outer Faint Veil.—Containing an account of this feature, with a reference to a similar peculiarity in the Comet, 1860, III.

XIV. On the Direction of the Initial Axis of the Tail.

XV. Conclusion.—This Section gives a general summary of the contents of the volume.

The naked-eye and telescopic views of the Comet, shown on a dark ground, are very beautiful specimens of steel engraving.

P. A. Hansen, Darlegung der theoretischen Berechnung der in den Mondtafeln angewandten Störungen; Erste Abhandlung. Leipzig, 1862. (forms part of t. vii. of the *Abhandlungen der mathematisch-physischen Classe der Königlich. Sächsischen Gesellschaft der Wissenschaften*, pp. 93–497.)

In this elaborate memoir—the first part of the anxiously expected Theory of Prof. Hansen's Lunar Tables—the perturbations in longitude are completely calculated; but of the perturbations of the latitude and radius vector respectively, only the portion which the author designates as the principal one. The method employed is stated to be with a slight variation that explained in the *Fundamenta Nova Investigationis orbitæ veræ quam Luna perlustrat*, the demonstration of the fundamental formulæ is however given *de novo*. It is hardly necessary to mention that the numerical values of the coefficients are substituted in the first instance in the formulæ, and the multiplications and integrations carried out numerically. The author has compared his values of the perturbations of the longitude with those of Plana and Damoiseau, the differences being in some instances not inconsiderable; but he has not as yet made the comparison with those of Delaunay. At the conclusion of the memoir the author investigates the perturbations which depend on the figure of the Earth, and the mass and figure of the Moon.

Nouvelles Recherches sur la Figure des Atmosphères des Corps Célestes. Par M. Edouard Roche. 4to, Paris, 1862. (Extrait des Mémoires de l'Académie de Montpellier, Section des Sciences, 1862.)

The present Memoir is a sequel to one published in the year 1854. The author, on the suggestion of M. Faye, has

since introduced into his calculations the hypothesis of a repulsion exerted by the Sun on the Cometary Atmosphere; the repulsion is assumed to act in the direction of the radius vector, and to vary as the inverse square of the distance from the Sun; but the intensity is supposed to depend on the matter operated upon, viz. that, while nothing for the nucleus, it becomes sensible or even considerable for the exceedingly rare atmosphere of the Comet.

The equation of the *couche de niveau* or level surface (which is of course a surface of revolution about the Sun's radius vector) is obtained in polar co-ordinates in a very simple form; for sufficiently small values of the parameter which determines the particular surface, the section of the surface is a sort of oval, elongated however on the side away from the Sun. Geometrically, however, there is a detached infinite branch of the section; and, when the parameter acquires a certain limiting value, the two branches unite together so as to form a curve with a double point on the side opposite the Sun; the *surface* has then a conical point. The physical conclusion is, that as soon as we come to this limiting form of the level surface, the atmosphere, instead of continuing to form finite envelopes surrounding the nucleus on all sides, will, so to speak, flow out through the conical point of the surface, and so give rise to a tail. Want of space prevents the giving a more complete account of this very interesting investigation.

Instrument to be disposed of.

A Fraunhofer Equatoreal, with Circles read by two sets of Verniers to 4^s and $1'$, in good order. Inquiries to be addressed to the Assistant Secretary.

ERRATUM.

Vol. XXIII., page 16, heading of Table C, *for* "in the year 1861," *read*, "the years 1854 to 1860."

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIII. *January* 9, 1863. No. 3.

Dr. LEE, President, in the Chair.

Major J. T. Walker, India;
H. M. E. Crofton, Esq., Inchinappa, Co. Wicklow;
Arthur J. Melhuish, Esq., 26 Haymarket; and
Richard Munday, Esq., R.N., Dockyard, Malta,

were balloted for and duly elected Fellows of the Society.

Observations on Saturn. By Wm. Wray, Esq.

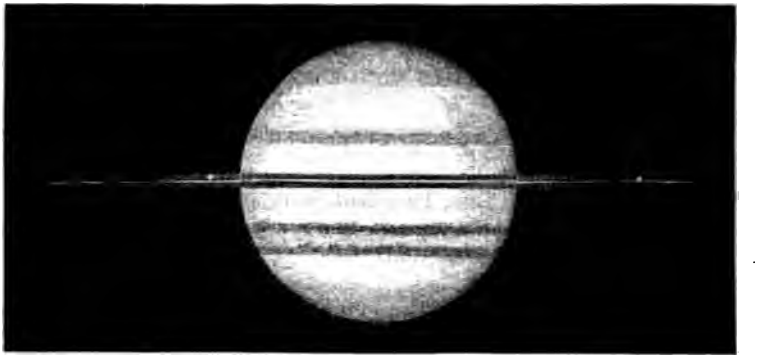
Telescope 7 inches clear aperture; 8½ feet focus.

1861, Dec. 17th, at $18^h\ 30^m$. On turning the telescope on *Saturn*, to my utmost surprise, with power 110, I instantly perceived the whole of the edge of the ring, not only where it crossed the dark shade on the body, but also extending on each side of the planet's margin. It was irregularly broken.

Dec. 23d, $17^h\ 20^m$. The edge of the ring again seen distinctly, and suspected to be thicker and somewhat nebulous about the region on either side, where it joined the planet's limb.

Dec. 26. Atmosphere very fine, and the image of *Saturn* exquisitely steady and well defined. The northern equatoreal belt was observed to consist of two narrow dark grey bands, connected by a broader region of a light brown colour: it thus had the appearance of being double. At $16^h\ 30^m$, the eastern arm of the ring was scarcely visible, whilst the western could not be overlooked, and was broken in two places. The

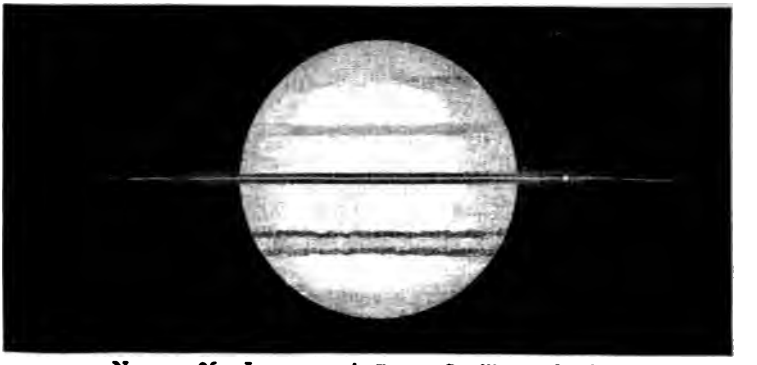
brightness of the eastern arm gradually increased; and at $18^h\ 30^m$ both were sensibly equal in light. At 19^h the eastern arm surpassed the western, and disappeared last in the twilight. A prolongation of very faint light stretched on either side from the dark shade on the ball, overlapping the fine line of light formed by the edge of the ring, to the extent of about one-third its length, and so as to give the impression that it was the dusky ring, very much thicker than the bright rings, and seen edgewise, projected on the sky. At $18^h\ 30^m$ I made the rough sketch, No. 1.



No. 1. 1861, Dec. 26, at $18^h\ 30^m$; two Satellites on the ring.

1862, Jan. 4th, $18^h\ 40^m$. Western arm much broken and brighter than the eastern, the faint overlapping light again seen.

Jan. 5, $13^h\ 30^m$. Edge of the ring well seen. Sketch,



No. 2. 1862, Jan. 5, at $14^h\ 0^m$; one Satellite on the ring.

No. 2, was made at 14^h; the faint overlapping light was exceedingly distinct at this time. The western arm of the ring

much fainter and shorter than the eastern, which was proved by its becoming invisible on raising the power from 110 to 450, the eastern arm remaining visible under the latter power. At $16^h\ 40^m$ the arms were judged to be equal in length and brightness, and both visible with power 450. At $17^h\ 15^m$ and until $18^h\ 0^m$ the western arm was decidedly the brightest.

Jan. 11th, $14^h\ 0^m$. The edge of the ring again well seen, and the faint light before referred to unmistakeably visible.

Jan. 18th, $13^h\ 30^m$. The edge of the ring broken, and the arms equal in brightness and length; faint light again clearly perceived near the planet's margin.

Shortly after these observations the instrument was sent to Archdeacon Garbett, of Clayton, in Sussex; and on the 19th of May, at $10^h\ 30^m$, that gentleman and his sons clearly perceived the whole of the edge of the ring of *Saturn*.

The fine thread of light seemed to me always of a bluish-white tinge, very different in colour to the ball of the planet. The inner satellites were frequently perceived moving along the fine line of light; *Mimas* distinctly on two occasions.

1 Clifton Villas, Highgate Hill,
Upper Holloway.

On the Observations of Saturn made at Pulkowa and Greenwich. By the Astronomer Royal.

A paper has lately been received from M. Otto Struve, dated 1862, July 2, containing numerous details on the appearances presented by *Saturn* about the time of disappearance of his ring in May of that year.

It is not my intention here to transcribe the whole of M. Struve's observations. I shall only remark that M. Struve considers himself justified, by the views which he as well as M. Winnecke obtained with the Pulkowa Refractor, in assuming the existence of certain appendages (*appendices lumineux*) which are not the same as the ansæ of the ring.

No such idea suggested itself to the observers with the Greenwich Equatoreal.

I have requested Mr. Carpenter to draw up an account of his observations, and I ask leave to place them before the Society in juxtaposition with extracts from the remarks of M. Otto Struve. On the whole the accordance is good.

It will be remembered that "left" in Mr. Carpenter's language corresponds to "preceding" in M. Otto Struve's.

J. C.

1862. May 5.

Right ansa longer by $\frac{1}{10}$ than the left.

May 13.

Ring of a dirty copper colour.

(A drawing was made.)

May 17.

The ring beyond the planet at times just visible on the left of the disk, but on the right only a small faint spot could be seen in the plane of the ring, about $\frac{1}{3}$ the diameter of the disk from the planet's limb. I should have suspected this was a satellite but for its elongated shape.

Sky very hazy.

Struve.

1862. May 15.

The line of ansæ perfectly well visible. On the north side it is more glittering, and limited by a sharp edge. On the south side, on the contrary, it appears as if clouds of a less intense light were lying on the ansæ.

On the body of the planet the obscure ring is visible, but I cannot see traces of the above-mentioned clouds.

Measures of Ansæ.

Preceding, 11″·07;
following, 10″·35.

May 16.

Preceding ansa without doubt longer than following ansa in the proportion of 5 to 4.

May 17, 15^h.

The preceding ansa estimated to extend 11″·4, or at most 13″·3 beyond the planet.

The light of the preceding ansa is of the colour of rose copper, and considerably stronger on the side touching the planet.

(Computed time for disappearance of ring, May 17^d 19^h.)

May 18.

I see the ansæ distinctly; they offer the same appearance as yesterday.

May 19.

Saturn very well seen. The ring distinctly visible on the left of the disk; and fairly, but not so distinctly visible on the right. Where it crosses the disk the under edge is much sharper and better defined than the upper edge.

(A drawing was made.)

May 19.

I see distinctly traces of the ring, especially on the preceding side.

The luminous appendages extend on the preceding side to 0·6, and on the following side to 0·3 of the diameter of the planet.

Their colour differs much from the ordinary colour of the ring. They are not yellow, but more of a livid colour, brown, and blue.

May 20.

Saturn very well seen; the ring distinctly visible, appearing brighter than it did last night.

(The drawing made last night may be considered as a fair representation of the planet's present appearance.)

May 20.

The aspect of the luminous appendages has not varied since last night, with this difference only—that I estimated the length of the preceding side as 0·65, and the following as 0·5 of the planet's diameter.

May 21.

Images good.

Estimated extent of luminous appendages:—Preceding side 0·6, following side 0·4, of the planet's diameter; also the intensity of the light appears much more feeble on the following side. The size of these appendages increased in the neighbourhood of the planet, giving them the form of sharp wedges.

May 22.

Images less favourable than yesterday. Extent of luminous appendages on preceding side 0·6, on following side 0·5 of the planet's diameter.

May 25.

The ring still just visible.

June 1.

The ring still visible during moments of good definition.

June 3.

Saturn well seen. The ring very plainly visible. During moments of very good definition I fancy I can see the flat surface of the ring very faintly illuminated.

June 3.

Image very bad, yet the luminous appendages are still distinctly visible. It appears that the length of the following ansa is a little the greater, but this is not certain.

December 22, 1862.

Mr. Joynson communicated to the Society a series of thirty-six drawings of *Mars*, taken at Waterloo, near Liverpool, Lat. 53° 28′ 24″ N., Long. 3° 1′ 44″ W., accompanied with the following explanation:—

The diagrams are selected from a considerable number of drawings of the planet *Mars*, about the time of his opposition, and extend from the 25th of Sept. to the 10th of Nov., 1862. They were taken with a lens of 3½ inches aperture, and a power of 370, both lens and eye-piece being made by Messrs. J. Cooke and Sons, of York. They are arranged so as to show the appearance of the planet during the whole time of its rotation, and a few are repeated so as to show that the aspect of the planet was not changed during the time the observations were continued.

The results of the observations appear to be,—

1st. That the band is continuous round the planet, with the exception of the place where the pale yellow stripe crosses it, as marked in the diagram of 5 October.

2nd. That the snow at the south pole is not exactly on the axis of rotation.

3rd. That the time of rotation is $24^h\ 37^m\ 49^s$.

No opinion is offered as to the permanency or otherwise of the band, but it is found that the disk continues (Nov. 1862) to show the appearances that it did at first, so that there is no difficulty in knowing what part has passed and what has to be seen later in the night.

The general colour of the planet (beside the band) was yellow, save a part in the northern hemisphere, seen between the 12th and 24th October, which was reddish. Sometimes the band had a dead greenish cast, and sometimes it was of a purplish line, but in the main it was very dark. On the 26th October the opening from it appeared to extend very nearly to the snow at the south pole, and then to form a darkish semicircle about it.

It may be remarked, that in general the northern hemisphere to the northern edge of the band has been the best to define, whereas the southern hemisphere has mostly been rather indistinct, and the yellow colour less perceptible than in the northern. The small circle on each diagram marks the position of the snow.

The planet has now become gibbous, and is both smaller and worse to define with the same eye-piece than when the observations were commenced.

The telescope is equatoreally mounted, and was driven by clockwork during the observations.

The eye-piece was the ordinary Astronomical Inverting kind.

Liverpool, 18th Nov. 1862.

Note on U *Geminorum.* By J. Baxendell, Esq.

On the night of 26th Dec. 1862, I was surprised to find U *Geminorum* shining as a star of the $10\frac{1}{2}$ magnitude, although, according to calculation, it ought at that time to have been nearly in the middle of its phase of minimum brightness, and therefore below the 14th magnitude; but on the 29th, and again on the 30th, it was invisible in a sky which permitted stars of the $12\frac{1}{4}$ magnitude in the immediate neighbourhood to be seen. The maximum must therefore have occurred previous to the 26th; and fortunately, owing to the kindness of Mr. Knott, I am enabled also to state that it was subsequent to the 20th. In a letter from this careful observer, dated January 6, 1863, he says, "I was not fortunate enough to obtain any observations of the recent unexpected maximum of that *most eccentric* variable, U *Geminorum*. I find from my journal, however, that I *looked for it* on December 20th, *but to no purpose*, though Mr. Pogson's '*g*' and '*h*' were well in view. The night was but indifferent, but I think we may safely assume the variable to have been below 12·3 or 12·5 mag." Mr. Knott also remarks that his observation, combined with mine, seems to show that either the changes were *very rapid*, or that the maximum was *rather a faint one*.

The occurrence of this maximum is, I believe, the greatest deviation from the usual course of the changes of U *Geminorum* that has yet been observed; and it shows very strikingly the importance of keeping a constant watch over variables of this class. It also shows the necessity of a material modification of the elements derived by Mr. Pogson from the early observations of this Star.

With reference to Mr. Knott's remark, "that either the changes were *very rapid*, or that the maximum was *rather a faint one*," it may be observed that a maximum of equally short duration occurred in November, 1858. On that occasion the variable attained the 9·3 magnitude, and yet the whole time of visibility with ordinary telescopes did not exceed *nine* days.

Manchester, January 8, 1863.

On the Elchies Equatoreal. By G. Knott, Esq.

Mr. Knott, in a letter dated Woodcroft, Cuckfield, Jan. 8, 1863, after referring to the quadruple star 222 P. I. *Arietis* (one of the objects examined by the Astronomer Royal for Scotland with the Elchies Refractor), writes as follows:—

"It has occurred to me, however, that it may not be uninteresting to the Members to learn that all four components of that group are very readily (I had almost said easily) visible in my 7½-inch Alvan Clark refractor, and that I should be inclined to attribute (as has been remarked by Mr. Dawes) any difficulty in catching up the small star 'B' less to its faintness than to its proximity to the brighter star 'A.' We have an illustration of this same principle in the case of the celebrated 'sixth star' in the nebula of *Orion*, for securing which sharp definition is far more necessary than great light in a telescope.

"From observations on Dec. 10, 12, 15, 20, I estimate the magnitudes of the members of the group (222 P. I.),

A 9·4 : B 11·3 : C 10·1 : D 6;

or reducing to Prof. Struve's scale (which is also that of Mr. Dawes),

A 8·5 : B 9·9 : C 9·1 : D 6

"A comparison of these with Mr. Dawes' values seems to indicate that I have rated them rather too high. A and C, however, accord well with Σ's estimates in the *Mens. Microm.*

"A rough set of wire-micrometer measures, on Dec. 12, yielded the following results, which are, of course, approximate merely:—

A B	Angle of Position	50° ±	Distance	2″½ ±	Estimated.
A C	—	166·4	—	37·3	
A D	—	360·8	—	133·6	

"By differentiating D with β *Arietis*, I at once recognised it as the star observed by Piazzi (*Hora* i. No. 222), and was not long in coming to the conclusion that, somehow or other, the magnitudes of A and D in the *Bedford Catalogue* had been interchanged. This little oversight being rectified, the magnitude values of the various meridian and extra-meridian observers are found to be in admirable accordance, and the invariability of the group settled apparently beyond a doubt."

Observations of Comet II. 1862, *taken at the Observatory of J. Gurney Barclay, Esq., at Leyton, Essex, and communicated by him.* By Hermann Romberg.

The following observations of Comet II. 1862 were taken with the ring-micrometer on the large Refractor, and are corrected for refraction and parallax:—

1862.	G.M.T. h m s	Comet's R.A. h m s	Comet's Decl. ° ′ ″	
July 31	10 53 28·8	5 54 13·92	+73 2 36·4	*a*
Aug. 1	13 1 31·2	5 58 56·46	73 39 19·1	*b*
3	12 44 8·5	6 9 25·21	74 52 0·3	*c*
5	9 25 37·0	6 22 17·11	76 4 58·8	Meridian.
7	9 38 38·5	6 41 30·61	77 31 38·7	*d*
14	11 15 55·6	9 46 35·95	82 0 29·3	*e*
15	12 3 3·9	10 38 50·71	81 55 3·0	*f g*
18	8 55 32·3	12 54 24·60	79 8 42·1	*h*
,,	9 4 17·9	12 54 37·06	79 8 7·7	*i*
22	9 29 10·3	14 35 48·21	68 58 10·7	*k*
,,	12 28 53·4	14 37 43·63	68 32 13·3	*l*
23	10 56 23·3	14 50 38·49	65 7 29·6	*m*
24	12 30 54·0	15 2 44·51	60 46 22·6	*n*
,,	13 21 2·8	15 3 6·47	60 37 19·0	*o*
,,	14 4 2·4	15 3 23·50	60 29 35·3	*o*
25	10 57 50·8	15 11 32·73	56 32 40·7	*p*
,,	11 30 52·4	15 11 44·36	56 26 4·9	*q*
27	12 16 19·9	15 26 37·13	46 0 31·0	*r*
28	11 55 26·4	15 32 33·15	+40 13 14·4	*s*

Mean Places of the Stars of Comparison for 1862·0.

	R.A. h m s	Decl. ° ′ ″	
a	6 1 15·42	+72 59 17·0	Arg. Zon. and Radcl. Cat.
b	6 0 11·71	73 45 11·0	A. Z.
c	6 4 39·03	74 53 38·3	,,
d	6 41 33·51	77 24 22·7	,,
e	9 17 8·06	81 55 53·5	Radcl. Cat.
f	10 26 29·11	81 51 16·1	Redhill Cat.
g	10 28 8·04	81 57 11·1	,,
h	12 51 46·93	79 14 51·3	A. Z.
i	12 56 29·83	79 10 56·9	,,
k	14 35 27·52	69 2 45·5	,,
l	14 36 25·48	68 37 1·8	,,
m	14 51 50·93	65 12 24·4	,,
n	14 58 13·51	60 44 51·1	Radcl. Cat.
o	15 0 13·46	60 33 51·7	,,
p	15 10 2·56	56 38 2·6	,,
q	15 12 20·76	56 31 10·2	A. Z.
r	15 27 18·48	46 6 6·2	,,
s	15 31 2·81	+40 17 15·3	Radcl. Cat.

I watched with great interest the very remarkable pheno-

mena presented by the nucleus, but was unable to take measures of the luminous streamer on account of the wire-micrometer being unfortunately under repair.

Mr. G. Barclay's observatory is situated at Leyton, in Essex, in longitude $0^h\ 0^m\ 0^s{\cdot}8$ W., and latitude $51^\circ\ 34'\ 34''$ N. This determination was obtained by Mr. John Worden, by connecting it with a point in the Trigonometrical Survey close at hand. The instruments are a fine Refractor, by Cooke and Sons, of York, of 10 inches aperture and 12 feet focal length. The mounting is upon the well-known German principle, and unites beauty of workmanship with great firmness and solidity. To the Refractor is attached a spider-line and double-image micrometer, the former being chiefly intended for measures of double stars. In order to facilitate these measures, an excellent driving-clock by the same maker was added in the past summer, which has given perfect satisfaction. It is proposed to measure, as far as practicable, the stars in W. Struve's last Pulkova Catalogue, Bessel's selected list, the known revolving double stars, and a number observed by Admiral Smyth in the Bedford Cycle. Observations of the fainter Small Planets it is hoped to procure, with a new wire-micrometer provided with illuminated wires in a dark field.

The Observatory contains also a Meridian-Circle by Troughton and Sims, the telescope having an aperture of 4 inches and focal length of 4 feet; the circles are 3 feet in diameter, divided to every five minutes, and read by four microscopes. The inclination of the instrument is determined by a level, and the errors of azimuth and collimation by observing northern stars and reversing. I hope to make a good use of this instrument, not only for the common determinations of time, longitude, and latitude, but for observations of Planets, and later for ascertaining the positions of Comparison-Stars.

The Sidereal Clock is by Simmons, and has a very satisfactory rate. In September and October I obtained some observations of *Mars* in opposition, using also the method recommended by the Astronomer Royal. The weather, however, was generally unfavourable. Since that time I have carefully examined the screw of the wire-micrometer, and have now measures of about 80 double stars, in part of which the double-image micrometer was employed. It is right to state that the mounting of the instruments was superintended by Mr. John Worden.

J. Gurney Barclay's Observatory,
Leyton, January 4, 1863.

Observations and Elements of Comet II., 1862.
By J. Tebbutt, Esq.

Mr. Tebbutt, in a letter dated Windsor, New South Wales, October 20th, 1862, writes:—

"I herewith enclose my latest observations of a large Comet which suddenly made its appearance here on the evening of the 1st September. The observations were made with a telescope of $3\frac{1}{4}$ inches clear aperture, and 4 feet focal length, provided with a ring micrometer diameter 20′ 50″. They are not so accurate as I could wish, owing to the faint and diffused light of the comet during the present month; they will, however, prove useful in the event of the comet not being more accurately observed elsewhere. The Rev. W. Scott resigned some time since his appointment as Director of the Sydney Observatory, but some observations have been made of the Comet by his assistant. I believe, however, they are not in a state to be forwarded to Europe by this opportunity. The physical changes of the Comet during the early part of September were very remarkable, for a brief notice of which I beg to refer you to a paper read by me before the Philosophical Society of this colony on the 8th instant. The paper is published in the *Sydney Morning Herald* of the 9th instant, a copy of which I forward with this letter. I have deduced the following elements from observations extending from the 1st September to the 15th instant.

Perihelion passage, 1862, August 23^{d}·1340 Greenwich Mean Time.

Perihelion distance	0·96290	
Distance of perihelion from ascending node in the direction of motion	152° 49′ 48″	
Longitude of ascending node	137 13 36	Mean Eq. of 1 Jan. 1862
Inclination of orbit	66 9 36	

Motion retrograde.

The most remarkable feature of the Comet's orbit is its near approach to the Earth's path at the descending node; according to my calculations the distance between the two orbits at that point is only half a million of miles. This Comet cannot be identical with that discovered by M. Tempel at Marseilles on the 2d July.

1862.	Greenwich Mean Time.	Star of Comparison.		Comet—Star. R.A.	Comet—Star. N.P.D.
	h m s			m s	′ ″
Sept. 19	21 49 48	B.A.C.	5538	− 8 34·2	− 3 4
	22 13 7	,,	,,	− 8 33·4	− 1 57
	22 37 29	,,	,,	− 8 32·4	− 0 55

1862.	Greenwich Mean Time. h m s	Star of Comparison.	Comet — Star. R.A. m s	N.P.D. ′ ″
Sept. 25	23 27 28	B.A.C. 5593	−11 16·1	−11 43
26	0 7 2	,, ,,	−11 12·6	−11 20
	22 5 51	,, 5583	− 9 30·3	+ 9 6
	,,	,, 5584	− 9 38·3	+ 9 26
	22 21 15	,, 5583	− 9 30·4	+ 9 18
	,,	,, 5584	− 9 38·6	+ 9 29
Oct. 3	21 59 27	Brisbane 5754	+ 5 1·3	
	,,	,, 5799	− 1 37·3	
	22 6 10	,, ,,	− 1 36·5	− 3 16
	22 18 17	,, 5754	+ 5 3·7	+ 5 58
9	21 47 59	*a*	+ 0 26·6	+12 22
	,,	*b*	− 2 11·6	+15 4
	21 54 41	*a*	+ 0 26·5	+12 14
	,,	*b*	− 2 11·6	+15 3
	22 7 33	*a*	+ 0 27·1	+12 21
	,,	*b*	− 2 11·4	+15 10
	22 15 3	*a*	+ 0 26·1	+12 47
	22 19 30	*a*	+ 0 25·6	+12 45
	22 23 46	*a*	+ 0 26·6	+12 36
	22 29 45	*a*	+ 0 25·8	+12 38
	22 34 37	*a*	+ 0 24·6	+12 42
			a — B.A.C. 5472	
			+17 36·7	+ 8 3
			b — B.A.C. 5472	
			+20 14·1	+ 5 11
12	21 40 32	Lacaille 6907	+ 7 8·2	
	,,	B.A.C. 5558	+ 6 32·3	
	21 52 10	Lacaille 6907	+ 7 10·7	+11 40
	,,	B.A.C. 5558	+ 6 35·2	+11 7
14	21 19 20	,, 5561	+ 8 2·6	− 2 7
	,,	*c*	− 0 47·1	+16 25
	21 32 38	B.A.C. 5561	+ 8 4·0	− 2 31
	21 45 2	,, ,,	+ 8 5·3	
	21 56 40	,, ,,	+ 8 2·8	− 1 49
	,,	*c*	− 0 48·6	+16 42
	22 9 37	B.A.C. 5561	+ 8 5·8	− 1 49
	,,	*c*	− 0 44·1	+16 42
			c — B.A.C. 5561	
			+ 8 50·4	−18 31
			c — Brisbane 5850	
			− 1 23·0	+11 1

Long. of place of observation	$10^h\ 3^m\ 42^s$	East of Greenwich
Lat. of place of observation	33° 36′ 30″	South

Remarks.

Where the difference of N.P.D. is not given it is to be understood that one of the objects crossed the ring near its centre. The stars *a*, *b*, are of the 7 mag., and *c* of the 7·8 mag. B.A.C. 5558 is double; the *following* star was employed in the comparisons of the 13th October. The following are the mean places of B.A.C. 5538 and 5583 from observations made at the Sydney Observatory.

B.A.C. 5538, January 1, 1859, R.A. $16^h\ 27^m\ 6^s{\cdot}44$, N.P.D. 124° 57′ 39″·4 from six observations in 1859.

B.A.C. 5583, January 1, 1860, R.A. $16^h\ 34^m\ 3^s{\cdot}41$, N.P.D. 130° 50′ 49″·6 from one observation in 1860.

Places of Comet II. 1862, *observed at Armagh.*
By N. M'N. Edmondson, Assistant at the Observatory.

(*Communicated by Dr. Robinson.*)

1862.	Greenwich M.T. h m s	R.A. h m s	s	N.P.D. ° ′ ″	″
Aug. 27	11 6 36·49	15 26 18·27	+ ϖ × 0·05596	43 41 54·28	− ϖ × 0·52008
28	9 16 17·63	15 31 41·87	0·04292	48 52 52·70	0·39972
29	9 4 21·21	15 36 46·30	0·04231	54 41 45·17	0·46049
31	9 32 36·89	15 45 8·31	0·03666	66 38 39·70	0·63061
Sept. 1	8 52 44·77	15 48 33·28	0·03132	72 14 58·45	0·66010
7	8 44 41·06	16 3 10·20	0·03038	99 50 47·09	0·86420
8	8 36 46·11	16 4 52·93	0·03009	103 7 5·87	0·87828
9	8 32 25·68	16 6 59·92	+ ϖ × 0·03019	105 57 15·46	− ϖ × 0·88794

	Comparison Stars.	Comparisons.	Assumed Places.
Aug. 27	Lalande 28208	5	Lalande.
28	52 Boötis	5	Armagh Catalogue.
29	P. xv. 148	6	R.A. from P., N.P.D. from Armagh.
31	Lalande 28875	4	Lalande.
Sept. 1	P. xv. 203	6	R.A. from Armagh, N.P.D. from P.
7	15 Scorpii	5	Ditto ditto B.A C.
8	Lalande 29671	5	Lalande.
9	Argelander Zone 297, No. 76*	6	Argelander.

* I am indebted to Mr. Ridings, of Markree, for the name and places of this star. Its identity with the comparison star seems doubtful. The comet preceded the star in R.A. $4^s{\cdot}25$, and was north of it 5′ 42″·08.

Results of the Meridional Observations of Small Planets, and an Occultation of a Star by the Moon; observed at the Royal Observatory, Greenwich, in the month of December, 1862.

(*Communicated by the Astronomer Royal.*)

Themis (24).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Dec. 1	10 27 32·5	3 9 36·10	71 52 6·81
13	9 32 45·4	3 1 58·70	72 21 20·07

Urania (30).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1862, Dec. 1	13 13 7·1	5 55 37·90	63 44 12·28
10	12 28 9·1	5 46 1·55	63 51 29·48
11	12 23 5·2	5 44 53·37	63 52 41·11
13	12 12 56·2	5 42 35·81	63 55 9·49
23	11 22 17·2	5 31 14·04	64 12 10·92
29	10 52 29·7	5 25 0·97	64 24 51·72

All the observations of N.P.D. have been corrected for Refraction and Parallax.

Occultation of α Cancri by the Moon.

Observed on Dec. 10, by Mr. Criswick. The disappearance took place at the Moon's Bright Limb at $15^h\ 54^m\ 54^s{\cdot}8$, and the reappearance at the Dark Limb at $17^h\ 4^m\ 46^s{\cdot}9$ M.S.T.

Observations of Asia (67), *taken with the Equatoreal of the Liverpool Observatory.* By J. Hartnup, Esq.

1862.	G.M.T	R.A. (67)	Log. $\frac{p}{P}$	N.P.D. (67)	Log. $\frac{q}{P}$	Star of Comp. B.A.C.
	h m s	h m s		° ′ ″		
Dec. 10	9 50 43·4	3 34 14·84	−7·829	77 6 15·2	−9·8114	1241
10	10 20 37·7	3 34 13·76	−7·154	77 6 19·4	−9·8104	,,
10	10 50 32·0	3 34 12·51	+7·592	77 6 23·2	−9·8105	,,
11	10 23 55·2	3 33 28·76	..	77 9 15·0	−9·8108	,,
11	10 53 49·7	3 33 27·42	+7·727	77 9 17·5	−9·8114	,,
17	9 56 20·9	3 29 30·02	..	77 24 1·3	−9·8130	1087
17	10 11 18·4	3 29 29·39	+7·426	77 24 2·3	−9·8130	,,
17	10 26 16·2	3 29 29·02	+7·726	77 24 4·1	−9·8136	,,

The observations are corrected for refraction. The corrections to be applied for parallax in time and arc are represented by p and q. P is the equatoreal horizontal parallax.

The following are the assumed mean places of the stars of comparison for 1862, January 0:—

	R.A.	N.P.D.	Authority.
	h m s	° ′ ″	
B.A.C. 1241	3 53 2·34	77 54 9·45	Greenwich Observations.
,, 1087	3 23 15·49	77 32 20·39	,, ,,

Mr. Warren De La Rue exhibited at the January Meeting a photograph of the Moon, taken near the dichotomy, enlarged so as to correspond in size with the map published by Beer and Mädler, and called the attention of the Fellows to the great progress which has been already made in Photographic Selenography.

Mr. De La Rue stated that, when he first commenced to map the lunar surface, the only way of procedure possible in the then state of the photographic art was, after the original negative had been taken, to place it under the microscope, and make, with the aid of the camera lucida, an enlarged copy of such portions of it as might be required by hand. Latterly, however, a more direct method has been discovered, by means of which, as Mr. De La Rue pointed out to the Meeting, the magnificent photograph enlarged thirty-eight times from the original negative had been produced. In the first instance a positive on glass was obtained, nine inches in diameter, from the original negative of one inch in diameter; and at the second operation a series of negatives were procured on a scale of thirty-eight inches for the Moon's diameter.

Mr. De La Rue, while bearing full testimony to the valuable labours of MM. Beer and Mädler, pointed out that their map did not give a true representation of the lunar surface as a chart, it only *mapped* it, while the photograph does both; and, although the latter might not be quite so sharp in its outlines as might be wished, still one must be contented to lose something in exchange for the great magnification of the negative. The combination of favourable circumstances necessary for the production of a perfect photographic picture was then alluded to. Atmosphere, collodion, instrument, driving-clock, all must be perfect; and the very rare occasions on which this combination occurs may be judged of from the fact that of four hundred negatives taken by Mr. De La Rue during the last five years, about twenty only are capable of producing a perfectly sharp photographic record; the tremulous motion in the

image from atmospheric defects being the *main* cause of the difficulty.

Turning to another allied subject, the photographs of the Solar Eclipse, Mr. De La Rue referred to certain discrepancies (to which he had alluded in his Bakerian Lecture on the Total Solar Eclipse of 1860, read before the Royal Society), between the photographs taken by Father Secchi and his own, and stated that, on a recent visit to the Roman Observatory, he remarked a photograph differing considerably from those which have been distributed in this country, by the kindness of Señor Aguilar, who had accompanied Father Secchi. Father Secchi's copy bore a very much greater resemblance to Mr. De La Rue's than these latter; and, upon inquiry, he found that after the first few positives had been taken from the original negatives, they had been subjected to a process technically called "strengthening," which, in this instance, had completely blotted out all the minute detail at first recorded.

While Mr. De La Rue was in Rome, Father Secchi's copy of the first place of totality was enlarged, and he brought back with him to England a negative corresponding in size with those taken with the Kew Heliograph by himself; and he has found, on tracing both and comparing the tracings, that the similarity is complete, with the exception that two luminous prominences are seen in Father Secchi's photograph which were not visible from Mr. De La Rue's station; that is, the only discrepancies between the two photographs taken at the two stations could be explained by the greater covering of the Sun in the one or other direction by the parallax of the Moon, dependent on the observers' positions, with respect to the central line of the eclipse. J. N. L.

Memorie Astronomiche. Del Prof. Donati. (Published in the Annals of the Museum at Florence, 1862.)

(*Abstract by S. M. Drach, Esq.*)

The first memoir, dated August 1860, is on the Striæ of Stellar Spectra. After quoting Fraunhofer, in Schumacher's *Astron. Abhand.*, Part ii., 1823, and in Gilbert's *Annalen der Physik* (vol. lxxiv., 1823), on the spectra of stellar and planetary light rays, the author states that he used a large burning lens kept in the Museum since 1690, and whereon Targioni, Averani, and Sir H. Davy had experimented. This lens has a diameter of 0·41 metres, and a focal distance of 1·58 metres (15 and 62¼ inches). The lens was mounted parallactically on a movable stand: a piece with a fine slit was placed a short distance within the focus of the lens, through which the stellar rays passed. Within this lens-tube

is a cylindrical lens with its very short focus, coinciding with that of the great lens. The stellar ray then falls on a prism fixed on a graduated circle, to which is attached a small achromatic telescope (object-glass, aperture 0·024; focal distance 0·17 metre; eye-piece magnifies 12 times). There is also at the end of the great lens-tube an achromatic lens equal to the object-glass of the telescope. Thus the stellar rays are passed through the cylinder-lens, the tube-achromatic, the prism, and the telescope-lenses before reaching the observer's eye. In the focus of the achromatic telescope are two metallic bars, one fixed, the other movable by a micrometer-screw. A cylinder-lens was preferred to a simple slit, for the purpose of getting the two lateral segments of the fasciculus of rays, which are lost in the ordinary diaphragm. But as by changing the position of the star observed, as respects the direction of the optical axis of the collecting lens, we change the position of the focal straight line produced by the cylindrical lens, making it impossible to keep the said straight line always in the same position as regards the prism, the author placed a metallic lamina at a distance from the cylinder-lens, equal to the focal distance of the latter. In the said lamina is a thin longitudinal slit, through which one sees the light-line formed in the focus of the cylinder-lens; thus certifying, when that line is visible, that it is always in the same position relative to the prism.

The author then proceeds as follows:—

I observed by day the solar spectrum, placing over the great lens a cloth, so fixed that it transmitted only the requisite quantity of light, and fixed the prism *p* to correspond to one or the other of the solar striæ; then fixing the telescope, I turned the movable bar, till one of its limbs touched the said stria at its minimum, and I read the corresponding division of the micrometer-screw. Leaving everything in this state, I turned the great lens in the evening to the star I wished to observe, and by the micrometer-screw immediately obtained the position of the stellar striæ, referred to the solar one observed in the morning. Besides the position of the stellar striæ, I have tried to determine the extremes of the spectra themselves, for which see Table.* I denote the striæ of the stellar spectra by the Greek letters, α, β, γ, proceeding from the least refracted end of the spectrum. The unlettered striæ have not been measured, but simply estimated, as being very difficult. I have grouped separately the separate-coloured stars, giving in each group the first place to the most conspicuous striæ, and so in order; classifying them from Humboldt's *Cosmos*, and Schmidt, in *Astr. Nachr.* The Table indicates that the stellar striæ have a certain relation to the star's colour. Thus the white stars seem to have a family likeness; and so the yellow, orange, and red ones. There is

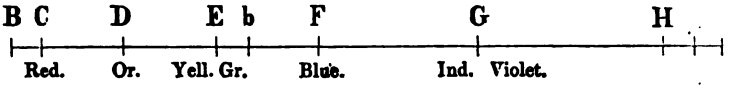
* See the figure, p. 103, roughly reduced from Prof. Donati's Memoir, and the numerical values, p. 104.—Ed.

likewise a great difference in the lengths. The three yellow stars have nearly the same maximum refraction end, but different minimum refraction ends; the reverse with the red stars. True, the measures of these ends (and indeed of the maximum refraction) are very uncertain, depending on the glass of the lenses and prism, and on the observer's eye. (Professor Amici has constructed a prism of very great dispersive power, without altering the axis of vision. It is composed of three prisms; the two outer ones of crown-glass, the inner one of boro-silicate of lead. Looking through this prism at a slit or luminous line, light is decomposed, with the same striæ as through a simple prism of flint-glass.) My prism was of flint-glass; refraction angle 60° 52′ 39″; angle of refraction of stria D of the solar spectrum (*i. e.* the angle which the refracted ray corresponding to D makes with the incident ray) is 49° 55′ 05″. The intermediate angles of the solar spectrum were found

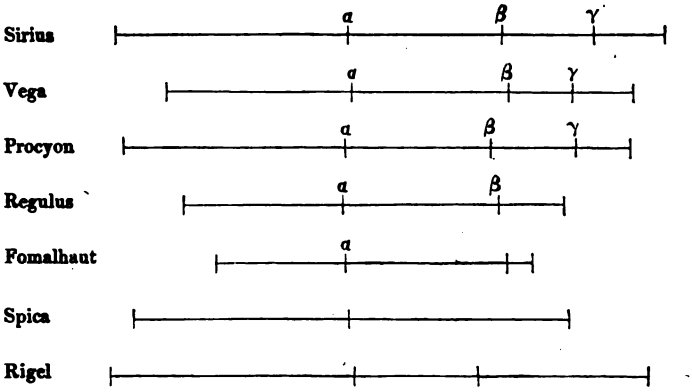
BC = 10′ 57″	Eb = 7′ 45″	FG = 72′ 9″
CD = 30 35	bF = 29 16	GH = 65 56
DE = 40 41		

All these angles I have measured by the theodolite as accurately as possible. The rim of the micrometer-screw = 11′ 5″; each of the 100 divisions of the head = 6″·65. Its movable bar was broad enough to be seen without illuminating the telescope; and I always placed the same limb in contact with the measured striæ. I observed when the star was close to the meridian, giving me more definite striæ; but we must not conclude that the striæ change with the altitude of the star; the horizontal scintillations naturally disturb the precision of the spectrum. Whilst observing, I tried to be in the dark as much as possible; ordering another to read the micrometers; for when the eye is fatigued, even by the feeble light of a lantern, it could not perceive the feeblest striæ of the stars. In cloudy or moonlight nights, the observations were uncertain and sometimes absolutely impossible. Although the great lens was not achromatic, yet, looking through the telescope at the slit illumined by the rays collected by the cylinder-lens, it appeared perfectly white and without perceptible coloured edges.

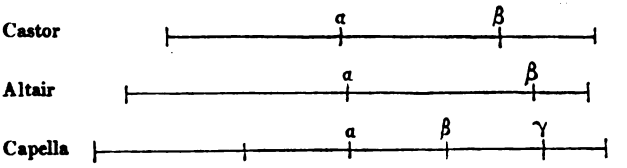
The following measures are the means of many observations thus obtained. Each star has its stria α referred to the solar F. Thus, for *Sirius*, $\alpha = F\odot - 15''$ means that *Sirius* α is refracted 15″ less than Sun F; and for *Vega*, $\alpha = F\odot + 40''$, that *Vega* α is refracted 40″ more than Sun F. $(r\alpha)$ means the angle between the minimum-refracted end of the stellar spectrum with its stria α; $(\alpha\beta)$, $(\beta\gamma)$, the angles between α and β, β and γ. (αv), (βv), (γv), are the angles made by α, β, and γ, with the maximum-refracted end v of its spectrum.



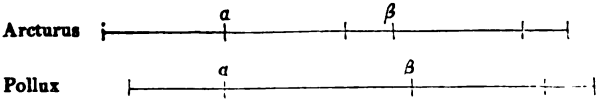
White Stars :—



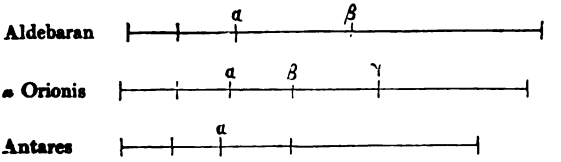
Yellow Stars :—

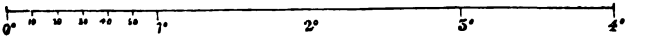


Orange Stars :—



Red Stars :—





A short description of each stria is appended, and its eye-estimated breadth referred to the micrometric bars. These said angles do not correspond to the central part of the striæ, but to their least refracted limb, which limb I always collimated with the movable bar of the micrometer. Thus *Sirius* α is refracted 15″ less than F Sun; but, since breadth of this $\alpha = 50''$, the central part of the said α is refracted 10″ more than F Sun. Similarly *Vega* $\alpha = F \odot + 40''$, breadth 40″, has its centre refracted 60″ more than F Sun, &c.

		$(r\alpha)$	$(\alpha\beta)$	$(\beta\gamma)$	(γv)
Sirius	$\alpha = F \odot - 15''$	85′ 40″	66′ 37″	42′ 27″	20′ 17″
α very fine, breadth 50″; β very fine, double, breadth 80″; γ as broad as α, faint.					
Vega	$\alpha = F \odot + 40''$	77′ 15″	67′ 56″	34′ 55″	10′ 18″
α, β, very fine; α 40″, β 60″ broad; γ very broad, faint.					
Procyon	$\alpha = F \odot - 33''$	78′ 55″	65′ 30″	37′ 1″	9′ 32″
α well seen, 15″ broad; β scarcely seen; γ ditto, rather broader than α.					
Regulus	$\alpha = F \odot - 60''$	68′ 30″	66′ 17″	30′ 55″	—
α very clear, 20″ broad; β seen at intervals.					
Fomalhaut	$\alpha = F \odot - 55''$	61′ 44″	70′ 36″	—	—
α fine, 35″ broad.					
Castor	$\alpha = F \odot - 30''$	69′ 43″	62′ 4″	30′ 9″	—
α very fine, 40″ broad; β ditto, 60″ broad.					
Altair	$\alpha = F \odot + 5''$	73′ 42″	66′ 3″	24′ 23″	—
α fine, 20″ broad; β indistinct, faint, 40″ broad.					
Capella	$\alpha = F \odot - 2' 40''$	81′ 1″	31′ 55″	34′ 35″	23′ 36″
α very thin, well observed; β, γ, scarcely visible, uncertain observation.					
Arcturus	$\alpha = F \odot - 29' 22''$	51′ 26″	44′ 27″	66′ 23″	—
α very thin, seen with trouble; β very difficult to be seen.					
Pollux	$\alpha = F \odot - 28' 42''$	47′ 33″	45′ 46″	68′ 56″	—
α very thin, seen with trouble; β very difficult to be seen.					

Aldebaran	α = F ⊙	− 22′ 29″	50′ 39″	29′ 40″	64′ 17″	—

α very fine, 30″ broad; β somewhat thinner than α, very fine.

α Orionis	α = F ⊙	− 30′ 35″	50′ 19″	17′ 24″	16′ 51″	57′ 18″

α very fine, 40″ broad; β very fine, 50″ broad; γ frothy, well seen.

Antares	α = F ⊙	− 33′ 28″	47′ 20″	18′ 17″	65′ 45″	—

α fine, 30″ broad; β rather fine, 20″ broad.

Now, comparing my results with Fraunhofer, there are very great differences. Thus, he says, *Sirius* has a very fine stria in the green, whilst I find this stria in the blue of the spectrum. He finds in *Pollux, Capella, α Orionis, Procyon,* a stria with the same position as D Sun, which line I have never seen. Perhaps attributable to the reason that the stellar light is never strong enough to distinguish the colours; saving a reddish tinge at the minimum-refraction end, they appear of a lavender tint in their whole length, which is always smaller than the solar one under the same conditions, from the great difference of light between the Sun and stars. If the stellar stria has not been measured, but merely eye-estimated (probably Fraunhofer's process), one may very easily get deceived, and mistake one stria for another. This occurred even to me at the beginning. Thus comparing the distance between the extreme red of *Sirius* and its α with that between the extreme red of the Sun, and E ⊙, I judged that α *Sirius* = E ⊙; but, passing to exact measures, I directly found that α *Sirius* = F ⊙.

Perhaps, as the stars change in colour, their stria-positions may change, or my more brilliant striæ may not have been the more brilliant ones to Fraunhofer's eye. (Thus Fraunhofer is silent on the difficulties of *Capella,* and speaks of the difficulties of *Procyon.* I found *Capella* very troublesome, and the stria of *Procyon* much clearer and sharper than any in *Capella.*) I merely point out the utility of making these observations at various times with the extremest accuracy, without hazarding any hypothesis.

However, I must remark that almost all my fifteen stars have a stria, very little differing from F ⊙. One may therefore presume that all these F-like striæ are constant, but with a greater or less refraction, from a difference in the refractive quality of the light of the particular star. On this hypothesis (partly agreeing with Fraunhofer's), it follows that the light of *Capella,* with a stria refracted 2′ 40″ less than F ⊙, must suffer a less refraction than the light of *Vega,* which is refracted 60″ more than F ⊙, whence the declinations of those two stars,

observed from two places, with very different (meridian) altitudes for each, must result differently. (Since $2' 40'' + 60'' = 220''$, and the refraction of F $\odot = 51^\circ 13' = 3073'$, with an horizontal refraction of $33'$. Hence the different refractions of these two striæ transferred to horizontal refraction $= \frac{33}{3073} + 220'' = 2''\cdot36$). I, therefore, compared Mr. Airy's *Cambridge Observations*, 1828–1835, with Mr. Henderson's, at the Cape of Good Hope, 1832–3 (*Mem. R. Ast. Soc.* vols. x. and xi.), which give these results:—

Camb.—C.G.H. 1830.		Mean Declination. Camb. 1830·0.	C.G.H. 1833·0.	A.P.M. in Decl.
″		° ′ ″	′ ″	″
+1·94	Fomalhaut	−30 31 13·92	30 19·1	−0·166
+1·71	Antares	−26 0 46·81	3 14·3	−0·034
+0·81	Sirius	−16 29 22·22	29 36·2	−1·198
+1·12	Spica	−10 16 15·60	17 13·7	−0·040
+0·57	Rigel	− 8 24 16·22	24 2·9	−0·012
−0·06	Procyon	+ 5 39 14·65	38 48·5	−1·023
+0·28	α Orionis	+ 7 22 3·72	22 7·1	+0·002
+1·32	Altair	+ 8 25 33·75	25 59·6	+0·380
+0·07	Regulus	+12 47 44·17	46 49·1	+0·006
−0·51	Aldebaran	+16 9 35·68	9 59·6	−0·174
+1·43	Arcturus	+20 4 16·41	3 18·0	−1·983
+0·72	Pollux	+28 25 45·50	25 20·4	−0·057
+0·49	Castor	+32 15 9·44	14 47·2	−0·078
+1·88	Vega	+38 37 50·56	37 57·7	+0·282
−2·00	Capella	+45 48 52·88	49 8·4	−0·425

I have extracted the Annual Proper Motion from vol. ii. p. 199, of *Annals of the Paris Observatory*. With them and the values in p. 181, to calculate the Precession, I have reduced the Cape Observations to 1830·0, whence I find as above. Whence *Vega* has at Cambridge a greater Declination than at the Cape of Good Hope, and *Capella* a less Declination at Cambridge. This seems to agree with my supposition, since *Vega* is low at the Cape with great horizontal refraction, and at Cambridge it is very high, with scarcely any refraction; and since the light of *Vega* seems from the above to be refracted more than the light of any other star, the refraction correction used at the Cape (which would increase the declination) is too small, making the star's declination less than that observed at Cambridge. The same horizontalities being verified for *Capella*, but, as contrary to *Vega*, the light of *Capella* is less refracted than that of *Vega*, the refraction correction

for the Cape of Good Hope must be too great for this star, causing a greater Cape of Good Hope declination than at Cambridge, as is the case. I chose the above two Catalogues, as being much esteemed and in approximate years: thus eliminating the uncertain influences of stellar Proper Motion. Also the stria *Vega* α is very fine, and *Capella* α, though thin and difficult, has been often observed by me with sufficient precision. This does not occur with Fomalhaut, which, rising to a very small altitude above Florence, and only seen for a short time, has been seldom observed by me, causing with its horizontal scintillations unreliable results. Indeed, my observations would lead to a contrary sign of the difference in declination between Cambridge and the Cape of Good Hope than to that which is actually obtained.

The sign for *Sirius* should be +, since the centre of its α is refracted 10″ more than F ⊙; the refraction correction applied to this star's observed place may be smaller than the true one, when the observed declination at Cambridge has not been increased enough, giving a smaller result at the Cape of Good Hope. The difference, *Altair* + 1″·32, also confirms my hypothesis; for, since the centre of its α stria is refracted 20″ more than F ⊙, one may suppose the horizontal refraction to be less than the true one, causing the Cambridge declination to be increased, and the Cape of Good Hope declination diminished; and, therefore, the former greater than the latter.

My measures are uncertain, more from the difficulty of hitting the exact stria-point than from the micrometer graduation stopping at 6″·6. The quickest way of reconciling the two Catalogues would be to make simultaneous observations at both places.

I would further recommend for observations of the stellar striæ a very large lens on Fresnel's principles for lighthouse lenses, as we do not so much require an image of achromatic sharpness as the concentrating of a great quantity of light, and such a lens would, perhaps, enable us to see as many stellar striæ as we find in the solar spectrum.

We further require to know the very small dispersive power in our atmosphere of the stellar light; also the stellar spectra breadth seen when near the horizon by a good telescope; also the maximum brightness point of various spectra. Such a series would be of first-rate value to modern Astronomy.

The second Memoir, dated February 1861, contains the author's observations of the Total Solar Eclipse of 1860, at Terrablanca, in Spain; and the third Memoir contains the Comet Observations at Florence, from 1854 to 1860.

Remarks upon the Phenomena attending the Disappearance, by Rotation, of the great Solar Spot of August 4th, 1862, as recorded by both Helio-photography and ordinary Telescopic Observation. By the Rev. F. Howlett.

Mr. Howlett, at the December Meeting, addressed the Society as follows:—

After the able remarks which have recently appeared in print respecting the comparative merits, in certain points, of celestial photography and telescopic observation—in connexion chiefly with the solar, and also the lunar surface—it might be deemed almost more than superfluous on my part to wish to bring the subject again before your attention this evening.

Mr. Dawes has alluded to the matter in a very instructive manner, both in an article which appeared in the *London Review* on the 18th of October last, and elsewhere; as also has Mr. Birt, in the columns of the same journal for Nov. 22 of the present year.

But the phenomena attending the disappearance, by rotation, of the very remarkable group of solar spots on the 4th of August last, and the observations then made, photographic and otherwise, in connexion with those phenomena, seem to bear so strongly upon one or two of the more *interesting* and at the same time most *warmly contested* points regarding the Sun's surface, that I hope I shall be pardoned for desiring to make you acquainted with the nature of a few of my own observations concerning the disappearance of this said group, as contrasted with the only photographic record of it which I have yet heard of, more especially since I was, I believe, the first to detect the discrepancy in this particular instance, in the course of a most interesting correspondence, which now for some little time has been maintained between Professor Selwyn and myself on such subjects; and I was permitted to make the case known in public on the 3d of October, before the members of the British Association assembled at Cambridge—as some here present may possibly remember.

It will of course be understood that it is in no carping spirit (which on every account would utterly unbecome me) that I venture to say that at times the photographic record may stand corrected by the simply telescopic one; the highest honours of this Society have recently, and doubtless most justly, been awarded to that distinguished member of our Society, whom we have now the great good fortune to own as one of our Secretaries, for his extraordinary success in applying the art of photography to celestial investigations of the highest interest; and earnestly do we all wish him every future success therein also, that both he and every lover of science could possibly desire.

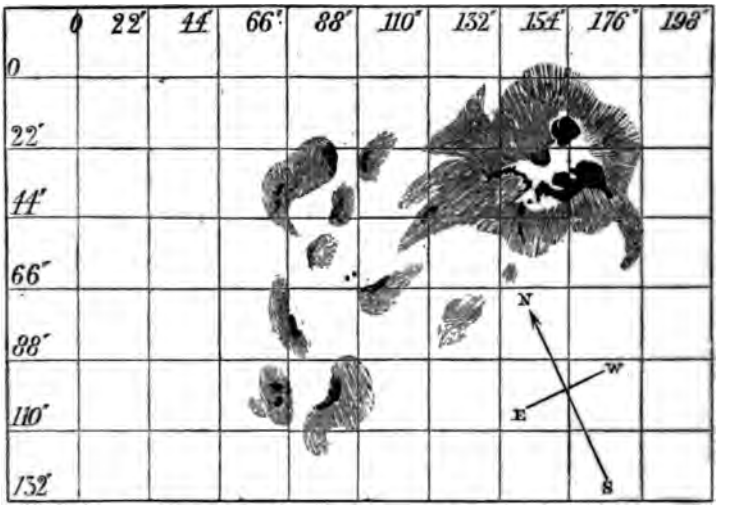
But not to be further tedious I would at once proceed to

remark, that the magnificent group in question (lettered ϱ in my record of Solar Spots), which passed off the disk on the evening of August 4th, was not observed by me before the afternoon of July 25th, by which time it had already advanced a considerable distance upon the Sun's surface. As seen near the centre of the disk, this group subtended about 4′ 30″ of arc, which would be equivalent to about 122,000 miles.

A carefully sketched series of drawings of the same, including twenty-two delineations, taken upon ten different days (and which are seen in sheets 83 to 86 inclusive), may be compared with the helio-autographs presented by Prof. Selwyn to the Royal Astronomical Society. Some of the drawings—those more particularly which illustrate the disappearance of the principal spot, and the photographic discrepancy on that occasion, have been re-copied in the diagrams on a scale of about ten feet to the solar diameter, which would give about four inches to one minute of arc, an amplification which, though not the best truly for distinct definition, may readily be obtained by the aid of a telescope of even not more than $2\frac{3}{4}$ inches aperture, and a power of about 160 diameters, by projecting the Sun's image upon a cardboard or other suitable screen, placed in a perfectly darkened chamber, at a distance of 8 feet from the eye-glass; and which arrangement enables me to observe and study the faculæ and solar surface generally, with remarkable facility and satisfaction. But the most effectual definition, amplification, and general management of the whole apparatus of both telescope, screen, and darkening shutter, are obtained in a simple but instructive method, by placing the screen on an easel, at the distance of about 4 ft. 2 in. from the eye-glass of my telescope—either one of the equatoreally mounted Sheepshanks' instruments, No. 4, belonging to the Royal Astronomical Society, $3\frac{3}{4}$ inches aperture, and 3 ft. 8 in. focal length, or a small Dollond of my own, of admirable definition and similar focal length, but of only $2\frac{3}{4}$ inches aperture, mounted upon a tripod stand, and armed with powers from 30 to 150 diameters. With a power of 80 diameters, the graduations of a kind of glass micrometer, ruled off into $\frac{1}{100}$ths of an inch, are seen beautifully projected upon a screen thus placed, (as also are any sun-spots or other solar phenomena that may chance to be in the field) each graduation meanwhile measuring half an inch in breadth, and comprising about 31 seconds of arc.

With regard, next, to the appearance of the group on July 25th, at $1^h\ 30^m$ P.M., I would direct attention to a remarkable mass of brilliant photospheric matter, at least 12,000 miles in length by 6,000 miles in breadth, (and embracing therefore a superficial area of not less than seventy-two million square miles), lying amidst the various associated nuclei of the principal spot, and completely insulated by a well-defined penumbra from the contiguous photosphere (fig. 1).

Fig. 1, 1862, July 25, 2·0 P.M.



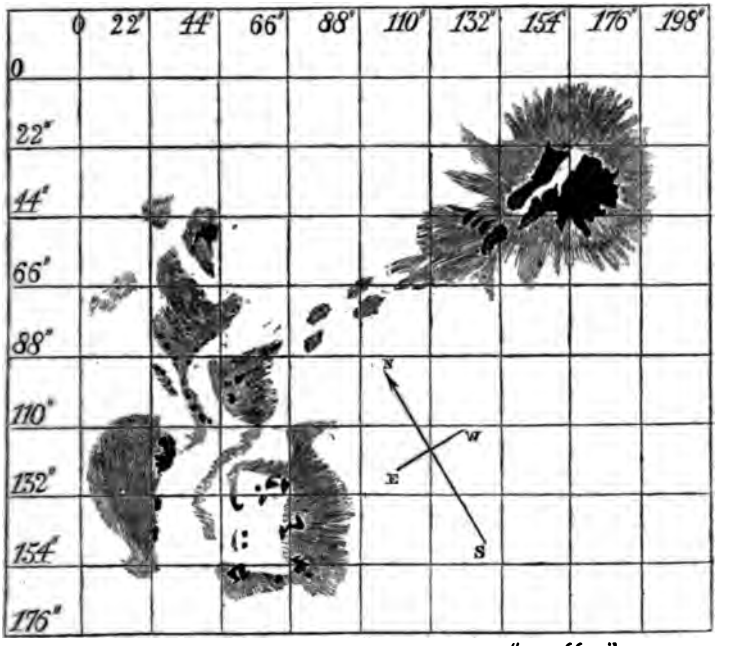
1″ = 466 miles.

On July 26th these nuclei had run into each other and become considerably enlarged, at the expense apparently of the bright patch; which last, either by sublimation, possibly, or a draining off sideways, or some kind of exhaustion, had become very much reduced in magnitude, as it was still more by $7^h\ 30^m$ A.M. on July 27th (see fig. 2), whilst, by $3^h\ 0^m$ P.M. on the same day, the patch was nearly obliterated, a slight hazy "bridge" alone remaining visible, and which in a feeble diffused form was faintly to be traced also on both the 28th and 29th July; but when I next observed the Sun on the 31st, this haze seemed to have entirely vanished. Some have thought that such a hazy film constantly accompanies a nucleus, but such is not my own impression. May not any extra appearanc of haze be rather accounted for by the occurrence of phenom similar to those which have just been described?

On July 31st, and August 1st, the great nucleus was gran fringed by projections from the contiguous penumbral ma often with sharp tooth-like terminations, and the disposit the subordinate spots and patches of penumbra was als interesting and instructive, as indeed they had been duri whole period of their apparition. But the highest p interest, perhaps, in connexion with this group, e itself on the occasion of the gradual disappearance r western limb on the evening of August 4th.

On the preceding day, indeed, the group, and m cially the principal nucleus, might be seen to be re foreshortened, and to be about to be divided again,

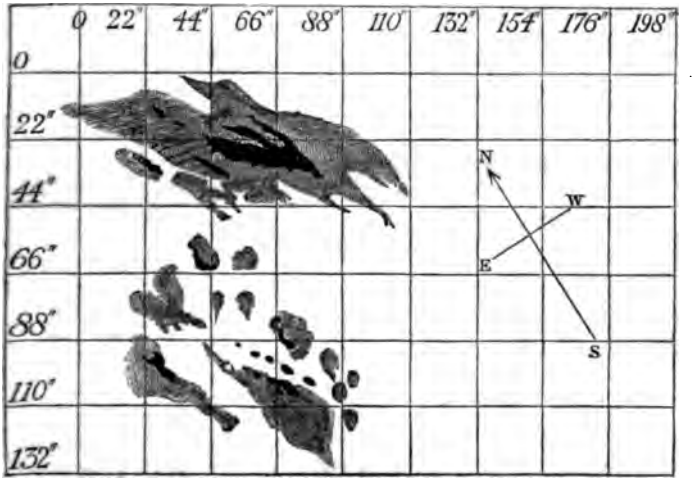
Fig. 2, 1862, July 27, 7·30 A.M.



1″ = 466 miles.

into two portions, by a sharply-defined and fresh "bridge," projecting across the chasm from the northern side (see fig. 3),

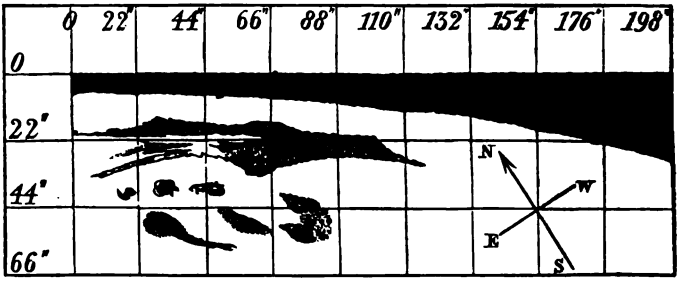
Fig. 3, 1862, Aug. 3, 6·40 A.M.



which feature (though springing then from the western side of the nucleus) had also characterised this spot during its earlier stages.

At $6^h\ 30^m$, on the morning of August 4th, the nucleus and "bridge," and many of the subordinate details, were still very distinct, though of course they were extremely attenuated, the nucleus then being, as nearly as I could judge, about twelve seconds of arc from the Sun's north-west margin; by far the greater portion of the penumbra, moreover, as Mr. Birt observed in his communication of October 20th last to the *London Review*, lying on its following or eastern side (see fig. 4).

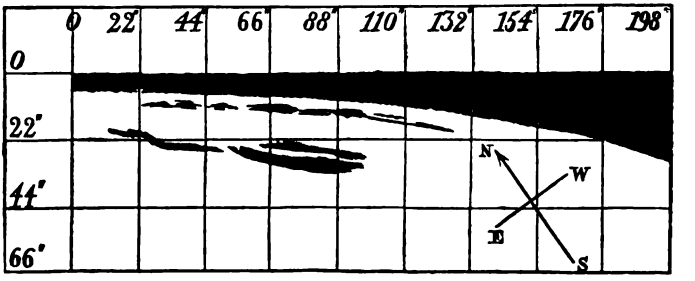
Fig. 4, 1862, Aug. 4, 7·0 A.M.



1" = 466 miles.

By $3^h\ 50^m$ P.M., the nucleus, now reduced to a mere line, yet (owing to the admirable state of the atmosphere that day in east Sussex) *clearly* to be distinguished from its adjoining penumbra, could not have been more than from 3" to 4" from the limb, which latter, however, as yet, was certainly not broken (see fig. 5).

Fig. 5, 1862, Aug. 4, 3·50 P.M.

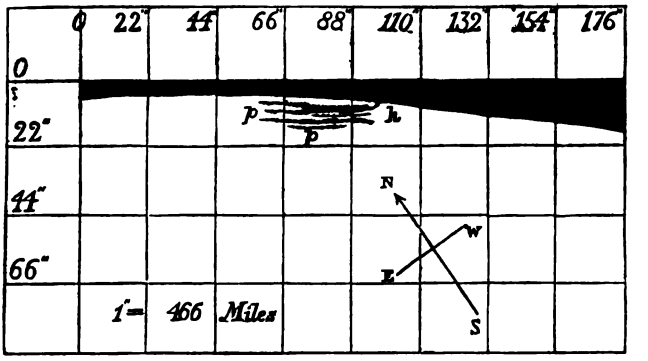


Pressing parochial business unfortunately prevented my observing the disk again till about 6 P.M., by which time the nucleus had completely disappeared, portions of its penumbra,

however, still remaining visible, as well as other patches of penumbræ adjoining.

And now it was that I am confident I could plainly perceive a *small notch* in the Sun's margin, precisely over the place where the great nucleus must have been existing (see fig. 6): and

Fig. 6, 1862, Aug. 4, 6 P.M.



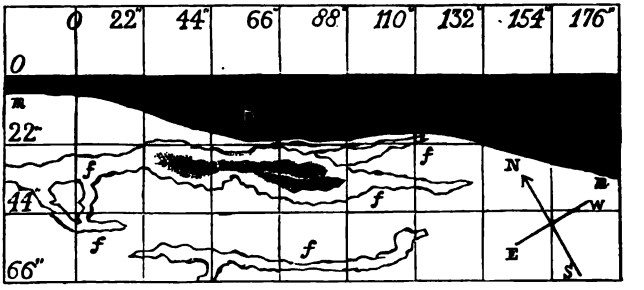
n, true notch in Sun's margin; *h*, hillock of facula; *p*, penumbral patches of great group.

it struck me that it was not caused so much by any deficiency in the circular limb of the Sun itself, but rather by reason of abnormally heaped up masses of the contiguous photosphere; which had perhaps been swept away, as it were, from the area occupied by the nucleus and penumbra, in like manner as a slight fall of snow might have been swept away from off a gravel path, and thus have been thrown up a little on each side. The portion of penumbra, in fact, which still remained visible, appeared to me to form the bottom of a shallow valley, seen obliquely over one of the elevations by which it was bounded, the right-hand, or south, bank (as seen in the inverted image) being the higher of the two.

Now all the foregoing observations had been communicated by me in a letter to Prof. Selwyn before I had any knowledge whether or no his photographer (Mr. Titterton, of Ely) had obtained "autographs" of the Sun on the day in question. A reply to my letter came to me shortly afterwards, in the form of two separate "autographs," taken respectively at the hours of $10^h\ 15^m$ and $11^h\ 30^m$ of the notable morning of August 4. But what struck me as very remarkable, was, that at the time when I had plainly observed and measured a brilliant streak of photosphere extending between the nucleus and the Sun's limb (and which I continued to observe and measure, at intervals, for nine hours afterwards), the autographs presented no appearance whatever of this streak, but

exhibited instead the appearance of a large notch or depression in the limb, approximately about 2′ of arc in length from N. to S., and 15″ or 16″ in depth from E. to W. See Fig. 7.

Fig. 7, 1862, Aug. 4, 10·30 A.M. (as represented in photograph).



1″ = 466 miles.

f stands for faculæ, P for pseudo gap in margin of Sun, *m* for normal margin of Sun; the subordinate spots only of the great group are yet visible, as shown in Figs. 4 and 5.

But this extraordinary depression, though hailed at the first by some of the advocates of the cavernous nature of the solar spots, as an almost triumphant proof of their views, went, as we have just seen, to prove *too much;* and, indeed, had the collodion been exposed to the solar rays for a somewhat longer period, in order that the comparatively dusky solar margin might have sufficiently depicted itself, no notch of any such dimensions (or, indeed, any notch at all) could possibly have appeared; and though Prof. Selwyn believes that this lengthened exposure would have spoiled, in all probability, the middle portions of the photograph (where, for instance, in sheet 86, the group lettered σ in my record is situated), yet in such a case it would only be requisite of course to take other negatives more especially adapted to the central parts of the disk.

Whilst on this part of my subject I would take the opportunity of mentioning that the helio-autographs of August 4th are not the only instances which present the appearance of such a pseudo-gap in the Sun's margin, whereby the existence of a large spot, when close to the limb, is completely ignored, sometimes.

Another such instance occurred in the photograph taken by Mr. Titterton on October 1st. Several of those who were attending at that time the meeting of the British Association, at Cambridge, would gladly have availed themselves of another of those rarely occurring chances of watching a large group fairly off the disk. Mr. Alexander Herschel did have such an opportunity at Collingwood, near Hawkhurst, that day, though

no notch was at any time apparent. Previously to leaving home I had requested Mr. Herschel to keep an eye on the group in question, and I received from him a letter, from which the following is an extract:—

"*Collingwood, Hawkhurst,*
"*Oct. 1st*, 1862.

"I send you in reply . . . a sketch of the Sun's appearance to-day. I missed the breaking of the white line, but the scale of the sketch, which is 1 inch to a minute, will show you that I saw it at $10^h\ 50^m$ A.M., to within 5 or 6 seconds of the Sun's limb, without any signs of the white line breaking."

And yet in the photograph taken that day at Ely the principal spot of the group is ignored, a shallow notch occupying its place, when, in point of fact, no such notch existed.

But notwithstanding any such discrepancies, allow me here to plead for the cavernous nature of the solar spots. For whilst jealously avoiding all desire to take any undue kind of argument from helio-photography, in its present stage, in support of the theory (which is also my own firm persuasion), that the solar spots are indeed cavities in the Sun's surface, I am not willing to be shaken in my belief that the photographic testimony bears most strongly in favour of that theory.

Photography, indeed, at times, would seem to wish to prove too much, as we said, but all its evidence, surely, runs in the same direction. It proves, surely, in agreement with the best eye observations, that the faculæ *undoubtedly* are raised billows or mountains, as it were, of photospheric matter, the crests of which (as Padre Secchi pointed out to me when lately at Rome) are necessarily not very visible when situated in the more *central* parts of the disk, where they are seen through the least perspective depth of the solar atmosphere, and where, in consequence, all similarly luminous photospheric matter, whether raised or otherwise, appears nearly alike brilliant; but which faculæ naturally exhibit themselves in far stronger relief towards the *marginal* portions of the disk, because that the rays which emanate from their lofty crests have to traverse, in their passage to our eyes, a far smaller amount of solar atmosphere than those rays have which emanate from their bases, and from the mean photospheric level above which the faculæ rise.

But may it not be argued thus, also, that not only are the faculæ elevations, but that the nuclei and penumbræ are depressions in the solar surface, and not clouds impending over it?

For if the solar spots were clouds, would they not (especially when anything like the size of the group of August 4th, 1862) exhibit much more frequent appearances of notches than they do?

They would surely be often seen in dark relief upon the Sun's limb, something after the way in which, at the beginning or close of a transit, the planets *Mercury* or *Venus* may be seen to produce an apparent indentation. And this appearance ought to take place, in the case of spots or groups of large magnitude, whether they were at a greater or less distance from the Sun's surface. Whereas, supposing them to be indeed cavities sunk in the spherical solar orb, then it seems clear that, even with the addition of the upheaved faculæ to enhance the effect, the phenomenon of a notch must be, *as it is*, exceedingly *rare*, and without the faculæ be well-nigh impossible.

But all the conditions requisite for forming the appearance of a small depression in the Sun's Limb were fulfilled on the evening of 4th of August. A group of unusual magnitude was passing off: there were mountain-like ridges of faculæ plainly conspicuous in the immediate neighbourhood of the same, and the eye of a spectator upon the earth would be looking along the shallow *trough* of the solar penumbræ, nucleus, and faculæ, in its greatest extent.

Another objection, finally, to the spots being cloudy masses appears to be this. Would it be possible for clouds to maintain such a comparatively *black tint* when immediately superimposed over the intensely incandescent surface (if it be incandescent, really, as well as brilliant) of the orb of the Sun?

RECENT PUBLICATION.

Sur deux Inégalités d'une grandeur remarquable dans les Apparitions de la Comète de Halley. Par A. J. Angström. (Extrait des *Actes de la Société Royale des Sciences d'Upsal*, Sér. iii. t. iv.) Upsal, 1862.*

The author remarks that Mr. Hind having succeeded in obtaining from the ancient accounts the appearances of Halley's Comet, as far back as the year 10 B.C., with considerable probability, he (M. Angström) was by these dates enabled to show the existence of two periodic Inequalities in the time of revolution of the Comet, surpassing as well in their magnitude as in the length of their period all the known Inequalities in the Planetary and Lunar Theories. The greater inequality, as regards the time of revolution of the Comet, is the more interesting, inasmuch as it depends on the known relation

* Mr. Carrington gave an account of this Memoir at the January Meeting, and he particularly called attention to the comparatively small amount of labour expended in obtaining such important results.—Ed.

(5 *Sat.* — 2 *Jup.* = 0 nearly) between the times of revolution of *Jupiter* and *Saturn*. The assumed data were—

Times of the Appearances of the Comet according to Mr. Hind.

− 10,20	451,50	912,25	1378,85
+ 66,07	530,84	989,70	1456,44
141,24	608,80	1066,25	1531,65
218,26	684,80	1145,30	1607,82 (N. S.)
295,25	760,44	1223,52	1682,70
373,84	837,26	1301,81	1759,19
			1835,87

thus comprising 24 complete revolutions, and giving a mean period

$$\tau = 76{,}92 \text{ Julian years};$$

or, taking the Julian year for the unit of time, the mean velocity of the Comet is $= 4^\circ\, 40'\, 48''{\cdot}72$.

Taking, then, the mean year 913,97 as the epoch, and the above value $\tau = 76{,}92$ as the mean time of revolution, this gives for the differences between the actual and calculated times of appearance, reckoned in Julian years, the series of values (already calculated by M. Mädler):—

−1,13	−0,95	−1,72	+3,27
−1,78	+1,47	−1,18	+4,04
−3,53	+2,51	−1,55	+2,31
−3,45	+1,59	+0,58	+1,48
−3,36	+0,27	+1,88	−0,46
−1,69	+0,21	+3,25	−0,89
			−1,13

These differences are laid down as ordinates in the plate annexed to the Memoir, the unit of distance for the abscissæ being the mean period τ, and that for the ordinates the half of a Julian year. And the form of the broken line joining the extremities of the ordinates, suggests the existence of the two inequalities (reckoned in Julian years):—

$$= 1{,}5 \sin\left((13 \text{ Com.} - 2 \text{ Jup.})\, n\tau + 13^{\circ}{\cdot}6\right)$$

$$= 2{,}3 \sin\left((\text{Jup.} + \text{Sat.} - 9 \text{ Com.})\, n\tau + 231^\circ\, 1'\right)$$

the former depending exclusively on *Jupiter*, but the latter on *Jupiter* and *Saturn* conjointly.

A numerical discussion of the data by the method of least squares, on the foregoing basis, leads to the corrected formula for the time of appearance:—

$$A = 913{,}97 + n\tau + 1{,}4626 \sin\left((13\ \text{Com.} - 2\ \text{Jup.})\, n\tau + 19^\circ\, 14'{\cdot}3\right)$$
$$+ 2{,}1642 \sin\left((\text{Jup.} + \text{Sat.} - 9\ \text{Com.})\, n\tau + 233^\circ 39'{\cdot}1\right),$$

with the corrected value $\tau = 76{,}93$, and resulting value $4^\circ\, 40'\, 46''{\cdot}6$ for the period and mean annual velocity of the Comet; and then with Hansen's values,

$$\text{Jup.} = 30^\circ\, 20'\, 56''{\cdot}7, \qquad \text{Sat.} = 12^\circ\, 36'\, 16''{\cdot}1,$$

the numerical values for the arguments are found to be

$$13\ \text{Com.} - 2\ \text{Jup.} = 8'\, 10''{\cdot}4 = 2649{,}7\ \text{Jul. years} = 34{,}44\ \tau$$
$$\text{Jup.} + \text{Sat.} - 9\ \text{Com.} = 27'\, 15''{\cdot}2 = 782{,}38\ \text{,,} = 10{,}30\ \tau.$$

The final comparison of the formula with the data is found to present a very satisfactory accordance. The author remarks that by the combined action of the two inequalities, *the time of appearance of the Comet might be accelerated or retarded* from one revolution to another, *by about a year and a half.*

Minor Planet (76). Discovered by Dr. C. H. F. Peters, at the Hamilton College Observatory, 22d Sept. 1862.

The following elements, calculated by him, are given (*Ast. Nach.* No. 1399):—

Epoch 1862, Nov. 0·0, Washington M.T.

		° ′ ″	
M	=	30 0 10·93	
π	=	334 40 12·13	Mean Equinox 1862·0.
Ω	=	359 52 19·09	
i	=	4 59 8·75	
φ	=	17 47 7·24	
μ	=	815″·1760	
log a	=	0·4258368	

Minor Planet (70) *Freia.* Discovered by Prof. D'Arrest, at Copenhagen, 14th Nov. 1862.

The following elements are given by him (*Ast. Nach.* No. 1397):—

Epoch 1862, Oct. 24·5, Greenwich M.T.

M	=	329° 35′ 4″·4	
ϖ	=	94 5 52·1	Mean Equinox 1862·0.
☊	=	213 58 4·3	
i	=	1 45 11·1	
φ	=	23 30 37·0	
log a	=	0·599052	
μ	=	448″·156	

A proposal has been made to form an Association for the Systematic Observation of Variable Stars. Members of the Society and others who may feel interested in the subject are respectfully requested to communicate with George Knott, Esq., Observatory, Cuckfield, Sussex, or Joseph Baxendell, Esq., 108 Stocks Street, Manchester.

Instruments to be disposed of.

A fine mahogany Telescope Tube suitable for a 7½-in. or 8-in. Object-glass, having a focal length of 9½ to 10½ feet; counterbalances, rackwork, illuminating apparatus, and finder; with a 2-in. object-glass, complete. This tube has been used for a 7½-in. object-glass, and was fitted up by Messrs. Dollond. Apply to Mr. Cooke, 31 Southampton Street, Strand, London, where the Tube may be seen.

The original Telescope and its Equatoreal Mounting of the late Dr. Pearson. The object-glass is nearly 7 ins. in diameter, and 12 feet in focal length. This Telescope is fully described and reported upon in the *Memoirs of the Royal Astronomical Society*, vol. ii. page 507. Apply to G. L. Prince, Esq. F.R.A.S., Uckfield, Sussex.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIII. *February* 13, 1863. No. 4.

THE Annual General Meeting of the Society, Dr. LEE, President, in the Chair.

Rev. Samuel F. Cresswell, Durham;
Major M. Foster Ward, Castle House, Calne, Wilts;
James L. Shuter, Esq., 33 Farringdon Street; and
William Esson, Esq., Merton College, Oxford,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Forty-third Annual General Meeting of the Society.

The Report of the Auditors, subjoined, will show the state of the finances:—

RECEIPTS.

	£	s.	d.
Balance of last year's account	171	9	1
By dividend on £2100 Consols	30	6	5
By ditto on £3850 New 3 per Cents	55	11	9
By ditto on £2100 Consols	30	6	5
By ditto on £3850 New 3 per Cents	55	11	9
On account of arrears of contributions	84	0	0
129 contributions (1862–63)	270	18	0
3 compositions	63	0	0
Carried forward	£761	3	5

	£	s.	d.
Brought forward	761	3	5
29 admission-fees	60	18	0
26 first year's contributions	44	2	0
2 contributions for 1864 and 1865	4	4	0
Sale of Publications	169	14	10
By interest on money for India Hill Observatory	29	16	5
	£1069	18	8

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Mr. Cayley, 1 year as Editor of the Society's Publications	60	0	0			
Mr. Williams, 1 year as Assistant-Secretary	100	0	0			
Ditto commission on collecting £631 14*s.* 10*d.*	29	5	0			
				189	5	0
Investments:—						
Purchase of £50 New 3 per Cents	46	9	0			
Ditto £50 New 3 per Cents	46	1	3			
				92	10	3
Taxes:—						
Income Tax, 1 year	1	17	6			
Ditto on E. I. Coupon	0	7	6			
Assessed and Land, 1 year	4	9	2			
				6	14	2
Bills:—						
Strangeways and Co., printers	243	12	0			
J. Rumfitt, bookbinder	36	5	3			
J. Basire, engraver	58	16	0			
Maltby and Sons, lithographers	37	18	10			
Williams and Norgate, printing	9	2	0			
Deville, gasfitter	4	12	6			
Insurance (Sun Fire Office)	12	5	6			
				402	12	1
Miscellaneous items:—						
Charges on books, and carriage of parcels	3	13	8			
Postage of *Monthly Notices*, letters, &c.	35	11	9			
Porter's and charwoman's work	24	1	3			
Tea, sugar, biscuits, &c. for evening meetings	13	13	0			
Waiters attending meetings	3	17	0			
Coals, &c.	12	0	0			
Sundry disbursements by the Treasurer	31	4	6			
Sundry payments out of Turnor Fund	5	13	0			
Deduction on country cheque	0	1	0			
Mrs. Jackson's annuity, 1 year	8	13	4			
				138	8	6
Balance at Banker's				240	8	8
				£1069	18	8

Assets and present property of the Society, Feb. 9, 1863:—

				£	s.	d.
Balance at Banker's				240	8	8
4 contribution	of 5 years' standing		42 0 0			
9 ———	of 3	ditto	56 14 0			
23 ———	of 2	ditto	96 12 0			
64 ———	of 1	ditto	134 8 0			
				329	14	0
Due for publications of the Society				2	1	8

£3850 New 3 per Cents. including Mrs. Jackson's gift (£300).
£2100 Consols, including the Lee Fund, (£100), and the Turnor Fund (£500).
Unsold publications of the Society.
Various astronomical instruments, books, prints, &c.
Balance of Turnor Fund (included in Treasurer's account) ... 69 16 10

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	21	X.	194	XXI. Part 2 (separate).	100
I. Part 2	63	XI.	204	XXI. (together).	124
II. Part 1	80	XII.	206		
II. Part 2	44	XIII.	227	XXII.	202
III. Part 1	98	XIV.	324	XXIII.	203
III. Part 2	119	XV.	193	XXIV.	210
IV. Part 1	120	XVI.	196	XXV.	219
IV. Part 2	131	XVII.	194	XXVI.	228
V.	147	XVIII.	193	XXVII.	483
VI.	168	XIX.	207	XXVIII.	449
VII.	191	XX.	200	XXIX.	482
VIII.	177	XXI. Part 1 (separate).	316	XXX.	262
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Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1862 ...	164	232	31	4	431	49	480
Since elected	3	21	...	1	...	1	...
Deceased	−4	−3	...	...	...	−3	...
Resigned	...	−3	...	...	...	...	...
Removed for non-payment	...	−2	...	...	...	...	...
February 1863 ...	163	245	31	5	444	47	491

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{1}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed, repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and $2\frac{1}{4}$-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. $2\frac{1}{2}$-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Wollaston* telescope, to Mr. Rees.
The *Lee* circle, to Mr. Burr.
The *Beaufoy* clock, to Dr. Booth.
The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument,		No. 1, to Mr. Lassell.
Ditto	ditto	No. 2, to Mr. De La Rue.
Ditto	ditto	No. 3, to Rev. C. Pritchard.
Ditto	ditto	No. 4, to Rev. F. Howlett.
Ditto	ditto	No. 5, to Mr. Birt.
Ditto	ditto	No. 6, to Rev. J. Cape.
Ditto	ditto	No. 8, to Prof. Wheatstone.
Ditto	ditto	No. 9, to Mr. Lockyer.
Ditto	ditto	No. 19, to Mr. Dayman.
Ditto	ditto	No. 25, to Mr. Dayman.
Ditto	ditto	No. 41, to Rev. C. Pritchard.
Ditto	ditto	No. 43, to Mr. Huggins.

The 6-inch circular protractor, to Mr. Birt.

Nothing definite has yet been ascertained respecting the other *Beaufoy* clock, the two invariable pendulums, and the quadrant (said to have been *Lacaille's*), reported for many years past as being in the possession of the Royal Society.

In consequence of a letter addressed by the Council to Lord Palmerston, Her Majesty's Government, in August 1861, granted the Society 1000*l.* in aid of the proposed temporary maintenance of an Observatory near Poonah, under the superintendence of the late Capt. Jacob. Upon that gentleman's departure for India, in the early part of last year, the sum of 500*l.* of this grant was advanced to him for his necessary expenses. Upon the Council receiving the information of the lamented death of Capt. Jacob, they directed application to be made to Lord Palmerston, for instructions as to the disposal of the balance remaining in their hands, amounting to 500*l.* In answer the Treasurer was requested to repay it to the Treasury, which has been done accordingly.

The Council have awarded the Gold Medal to Prof. Argelander, for his Survey of the Northern Heavens. The President will, in the usual way, explain the grounds of this award.

Printed Transactions of the Society.

The thirty-first volume of *Memoirs* is now published, and contains nine papers. Four of them are communications from Sir Thomas Maclear, referring to Comets which have been observed by him at the Cape of Good Hope, between the years 1853 and 1861. The fifth is by Mr. Cayley, and relates to the Problem of Disturbed Elliptic Motion, the object of the Memoir being to obtain the differential equations for the radius vector, longitude and latitude of the disturbed body, when the last-mentioned two co-ordinates are referred to an arbitrarily varying plane and departure point or origin of longitudes therein.

This is followed by three short papers by Colonel Strange, on certain interesting points connected with the adjustments and construction of Altazimuth Instruments and large Theodolites. These latter investigations may probably be opportune with reference to a considerable number of unusually large theodolites which are now in process of construction for the Great Indian Survey. The volume concludes with a Memoir, entitled "The Results of Planetary and Lunar Observations made at the Madras Observatory between the years 1831 and 1852." The reductions are made by Mr. Breen, after the manner and with the constants adopted at the Royal Observatory at Greenwich. Independently of its intrinsic value, this paper possesses an additional, though melancholy, interest, as associated with the death of Captain Jacob, under whose superintendence many of the observations were made.

Obituary.

The Society has to regret the loss by death of Prof. Barlow; Dr. Fisher; Thos. Gibbes, Esq.; Capt. Jacob; John Riddle, Esq.; Sir J. C. Ross; J. Walton, Esq., Fellows of the Society; and also of M. Carlini; Professor O. M. Mitchell; M. C. Rümker, Associates. The death of M. Biot, Associate of the Society, was announced in the last Annual Report, but the biographical notice was deferred till the present year.

Peter Barlow, Esq., F.R.S., Member of the Imperial Academy of St. Petersburgh, and of the Royal Academies of Brussels, Utrecht, &c. &c., was born in the parish of St. Simon, Norwich, in Oct. 1776, and died March 1st, 1862, in the eighty-sixth year of his age. In 1801 he was appointed one of the Mathematical Masters of the Royal Military Academy, Woolwich. In 1811 he published his first work, *On the Theory of Numbers;* in 1814, his *Mathematical Dictionary;* and in the same year, his "Mathematical Tables." Up to this time Mr. Barlow's pursuits had reference to abstract science. In 1815 he commenced his first attempt at the application thereof to physical science, founded on experiments on the strength of wood, iron, and other materials. This formed the basis of his work, *On the Strength of Materials*, published in 1817, which, being well received by the architects and civil engineers of the metropolis, led to an intimate acquaintance with the leading professors of those arts. It was also about this time that Mr. Barlow began his connexion with the *Encyclopædia Metropolitana*, to which he largely contributed. About 1819 his mind was turned to the pursuit of magnetic experiments, in which he was very successful, as well in developing the laws of the action of magnetism as in the application of those laws to the correction of a long-standing error in Navigation. These formed the subject of his *Essay on Magnetic Attraction*, pub-

lished in 1819. For this discovery he received several honorary and pecuniary rewards. In 1821 he received the Gold Medal of the Society for the Encouragement of Arts. In 1824, on the adoption of his corrections in the Russian Navy, his services were handsomely acknowledged by the Emperor Alexander. In 1825 he received the Copley Medal of the Royal Society, with pecuniary presents from all the principal Naval Boards.

The subject which had covered him with these honours, and which engaged much of his attention for seven years, having been fully established in the British Navy, as well as in the navies of several foreign powers, and no longer requiring his attention, he turned to optical experiments. In 1827 he suggested a new form of Refracting Telescope, of which he constructed two, and laid them before the Board of Longitude, with a proposition for constructing a large national telescope, which obtained the approbation of that Board. The great difficulty of procuring flint glass for achromatic telescopes seems to have been the principal motive for attempting this novel construction, the basis of which was an object-lens of fluid cased in glass.

Mr. Barlow was a sound mathematician, and was acquainted with the foreign writers at a time when this could not be said of many. He showed this acquaintance in his work on the Theory of Numbers—a subject on which he has, as yet, no English successor. His Tables—which give the squares, cubes, square and cube roots, reciprocals and factors, of all numbers up to 10,000—would have been exceedingly useful to all engaged in practice if the disposition had existed to believe in anything beyond a table of logarithms. As it happened, the use of them was confined to but a few of those whom they might have benefited. The late Mr. Henderson, for instance, who discovered for himself that Crelle's multiplication-table up to 1000 × 1000 is, for many purposes, a far more powerful instrument than the table of logarithms, was a constant user of Barlow's Tables. The work, now out of print, contains many subsidiary matters of use and interest. What is described above, the factors excepted, was republished in stereotype, under the sanction of the Useful Knowledge Society and the superintendence of Mr. Farley, in 1840.

For many years before his death Mr. Barlow had completely relinquished active exertion. Whether we look at his public or his private life, the Society may be proud of his name, which, owing to his great age and long retirement, belongs to a former day.

Capt. William Stephen Jacob, son of the Rev. S. L. Jacob, was born at Woolavington, in Somersetshire, Nov. 19th, 1813. He was chiefly educated at home, until he went as a Cadet to the Hon. East India Company's College at Addiscombe, in the

beginning of the year 1828, being then fourteen years old. Having passed for the Engineers in less than the time allowed for that course of study, he went, according to the regulations then in force, to Chatham, where his aptness as a mathematician still further developed itself. In 1831 he sailed for India, and not long after his arrival at Bombay he was appointed to assist General, then Lieut., Shortrede, who was engaged in the Survey of North-Western India, and in this occupation he was employed for several years.

During his stay in India, Capt. Jacob was not only assiduous in all his professional duties, but found time for his favourite study of astronomy, as evinced by the measures which he made of double stars at Poonah, with his own instruments in his own private observatory.

He came to England on furlough in 1843, and having married during his stay there, he returned to India again in 1845. A few years afterwards he resigned the Company's service, but he continued to reside in India, devoting himself to scientific pursuits, until he was appointed to the post of Astronomer at Madras, without application on his part, but from his several papers transmitted to this Society, and printed in their *Memoirs*. How admirably he discharged his duties there, as a careful and accurate observer, and able computer, is well known to us all, by the orbits which he deduced from his own observations of some binary stars, by his measures of *Saturn* and his satellites, and by his rigorous scrutiny of the mass of *Jupiter*. He found the climate of Madras, however, unsuitable to his health, and after having twice been obliged to return to England on sick certificate, he felt it his duty at last to resign the appointment. In the end of April last year Capt. Jacob, having purchased a telescope of nine inches aperture, returned to India under the auspices of the Astronomical Society, for the purpose of establishing an Observatory on the Mahratta hills. He landed at Bombay on August 8th, and started off for his former place of residence, Poonah; but, having been seized with a violent liver attack, expired there on the 16th of that month, in the 49th year of his age, leaving a widow and eight children.

Capt. Jacob was a man of deep religious feelings, and an earnest Christian. He seemed capable of mastering any subject to which he turned his attention, though mathematical science attracted him the most. His memory was singularly retentive, so that from his extensive reading his mind was remarkably stored with both ancient and modern lore.

Mr. John Riddle was born at Newcastle-upon-Tyne in November 1816, and was the only son of one of the earliest and most respected Fellows of our Society, the late Mr. Edward Riddle.

At the early age of fifteen Mr. J. Riddle was appointed an

Assistant Master in the Greenwich Hospital Schools, and on the retirement of his father in 1851 was chosen to succeed him as Head Master.

In the year 1846 he was elected a Fellow of this Society, and from that time till his death was a regular attendant at its meetings, and an occasional contributor to its papers.

He was appointed, in the year 1854, Examiner in Navigation to the Science and Art Department of the Committee of Council on Education, and held for many years a similar appointment in the Society of Arts. By both these Institutions his services were highly valued.

As a teacher Mr. John Riddle was perhaps unrivalled, and his success in instilling into the minds of mere boys not only the practice but also the theory of Navigation was very remarkable. The influence he possessed with his pupils was unbounded; and the good which he has accomplished by stamping on a vast number of the scientific officers of the Royal Navy the impress of his own vigorous mind so great, that his untimely death can be looked upon in no other light than a national loss.

Mr. Riddle was not only a good mathematician and a successful teacher, but also an accomplished gentleman, with a great taste for poetry and the arts. In society he was a universal favourite, and his urbane manners and intellectual conversation will long be remembered by a large circle of sorrowing friends.

His death was the result of an accident—a fall from a platform in his class-room, which produced concussion of the brain, from which, after lingering sixteen days, he expired on the 11th of October last.

Mr. John Walton was one of the members of the Mathematical Society, and became a Fellow of this Society when, in 1845, the two were amalgamated. He was for many years in business as a sugar-refiner, from which he retired in 1832 with a competency. He joined the Mathematical Society in 1806, and was much respected by the members. He was also a Fellow of the Linnean and Entomological Societies, to whose Transactions he contributed numerous valuable papers. He died at Knaresborough on the 3d of January, 1863, aged 78.

Jean-Baptiste Biot (born April 21, 1774, died Feb. 3, 1862) is the last of the great men of science who belonged to what he himself jocosely called the *first edition* of the French Revolution. To give any account of his labours on such an occasion as the present would be inadmissible: several of them are too well known, the others are too numerous. We shall therefore allude to but few of the 477 heads under which his writings have been enumerated by M. Lefort, the son of his

daughter's daughter, who declares himself certain that the list is far from complete.

The father of J. B. Biot, whose ancestors had been farmers in Lorraine, was himself in some office of the Treasury. The son, after studying at the Collège Louis-le-Grand, joined the army as a volunteer, and was present at the battle of Hondschoote, in 1793. He then retired from military life, and became a student in the Ecole des Ponts et Chaussées, and in the Polytechnic School. In 1797 he was named Professor of Mathematics at the Central School of the department of the Oise. He here introduced himself to notice by some mathematical papers, and became known to Laplace. In 1800 he was removed to Paris, as Professor of Mathematical Physics at the Collège de France, and Associate of the Institute. At his death he was a member of three of the four French Academies—a distinction which few, if any, ever gained before him. In 1806 he commenced, in concert with Arago, his continuation of the arc of the meridian. In 1808 he measured the length of the pendulum at Bordeaux. In 1817 he repeated this operation in Scotland and the Shetland Islands. In 1818 he was employed at Dunkirk, in concert with an English Commission, on the determination of the latitude. In 1824 and 1825 he was employed in Illyria, &c. on the measurement of the pendulum at different points of the mean parallel of latitude, and also at the extremity of the Spanish arc. These were the points in which a long life passed in physical investigation was most connected with astronomy. It is not necessary to speak in detail of the publications connected with the Great Survey. His work, entitled *Traité Elémentaire d'Astronomie Physique*, of which three editions were published (1805, 1810–11, 1841–57) was more than its name imports, being a mathematical system, in which the mathematical part is subordinated to a very extensive treatise on astronomy strictly physical: it is, in fact, what was left undone in the closely mathematical system of Délambre.

It is not our business to do more than refer to the extensive writings of Biot on optics and physics in general. All of middle age who were students of these subjects will remember that Biot was the originator of those encyclopædic works on experimental science which continue to appear from time to time. Four valuable volumes, the *Traité de Physique*, appeared in 1816; followed, in 1817, by the *Précis Elémentaire*, in two volumes. The reader of these volumes remembers how frequently Biot had to refer to his own experiments, and how completely he was made to feel that his instructor had travelled over all the ground, either by experiments of his own making, or actual and personal cognizance of those made by others. In these, as in all his other publications, there is continual evidence of the presence of the mathematician who has all the powers of geometry in reserve, but is prevented,

both by a natural turn for the experimental and by the matured habits of an old teacher, from introducing more than will just serve the purpose.

Biot must be numbered among the historians of science. Not that he ever wrote a work professedly on history, but because his writings on historical points form a large mass, involving much research and much valuable criticism. On the ancient Egyptian and Oriental astronomy he was engaged during most years of the second half of his life. In particular we may refer to his "Recherches sur l'Année vague des Egyptiens," read before the Academy of Sciences, 1831, where he shows, from the hieroglyphics in the Rhamesseum, at Thebes, that the point of the vernal Equinox was in the constellation *Taurus* during the reign of Rhameses III. He was editor of the *Journal des Savans* in 1816, and remained a contributor to the end of his life. He wrote in it, or in the *Moniteur Universel*, about 120 articles, many relating to the history of the past, and all of the critical cast. Thus we have, from 1816 to 1862, a succession of descriptive accounts, involving history or criticism from one mind. To this must be added twenty-three biographies in the *Biographie Universelle*, many of which are cited as separate works, and many short articles scattered elsewhere.

An account of the opinions of Biot, as involved in his accounts of others, would lead to discussions involving matters of controversy. The style in which he treats his subjects is eminently attractive. In his didactic writings he is diffuse, and his own peculiar spirit is undiscernible through his explanations. In his critical writings he has a strong and distinct character, marked by satire powerfully applied, but without ill-nature. In his private life he gained the respect of all. He was the Nestor of the Institute for many years. He was for three weeks the subject of Louis XV., and lived nearly ten years under Louis Napoleon. Of this long life, sixty-five years were passed in public posts.

Francisco Carlini, Director of the Royal Observatory of Milan, died at Milan on the 29th of August, 1862, in his seventy-seventh year.

He commenced his astronomical career at an early period, by the calculation of the *Milan Ephemeris* for the year 1804, and his highly useful labours extended over two-thirds of a century. During the whole of this period he prosecuted his studies for the advancement of science with unceasing assiduity.

He continued his calculations and writings in the *Milan Ephemeris* until his last illness, and only four weeks before his death he completed his calculations of the Elements of the second Comet of 1862.

Carlini's writings and his exertions for the advancement of

learning and science are familiar to all astronomers. He was conversant with foreign languages and literature, and spent much time on works of mechanism. His deportment was pleasing and agreeable, and his moral character blameless.

Of our late valued Associate, Professor ORMSBY MACNIGHT MITCHEL, it may be said the account of his life resembles a romance rather than a biography. He was one of those many salient instances of unconventional humanity who present themselves in a new country for the instruction, if not for the imitation, of their contemporaries. America may be justly proud both of the intellectual powers and the versatility of a man who, in early manhood, was a Soldier, then a Professor of Mathematics, then a Lawyer, then an Astronomer, then a Railway Engineer, and finally as a Major-General, ended his life, as he began it, in the Army.

Professor Mitchel was born on the 28th of August, 1810, in Union County, Kentucky. At an early age he gave promise of the ability and eminence which his future so fully realised. He was appointed a Cadet in the U. S. Military Academy in 1825. He graduated with distinction in 1829, and was very soon appointed Assistant Professor of Mathematics, which post he held for two years. He subsequently resigned his commission in the Army, and, after practising Law for a short time, he accepted the chair of Mathematics and Astronomy in the Cincinnati College, Ohio, in which position he continued for ten years.

It was here that he commenced a series of lectures; and those which he delivered on the physical constitution and telescopic appearances of the planets created so vast an enthusiasm, that it was resolved to furnish the city with a large telescope forthwith. Subscriptions for this purpose poured in from all quarters; and as specie was not always at hand, there were not a few contributions in kind, such as hats and caps, and other unastronomical apparatus. Mitchel then proceeded to England in quest of his telescope, and was there introduced to the Astronomer Royal by John Quincy Adams. At his earnest request, Mr. Airy allowed him to go through the greater part of the Greenwich course; and for some time he worked as a computer at one of the Royal Observatory desks. On his return home, the population of Cincinnati assembled, in the largest church of the place, to hear the account of his mission. The first-fruits of Mitchel's labours with his large equatoreal was the discovery of the companion of *Antares*. But, inasmuch as a rival telescope at another place, owing to an idiosyncracy of its object-glass, showed two companions instead of one, the ill-fated Astronomer and his Observatory were shorn of their glory and their subscriptions besides. After this, the Professor was concerned as Projector, Director, and Engineer, in "building" certain Railways; and it is hoped

he realised a good fortune thereby. He was in England a second time for the sale of railway bonds.

During his career as an Astronomer he was certainly one among the first who in any way applied galvanic electricity to astronomical observations; but this remark is not intended to apply to the principle of the *chronographic* record.

In the course of the unhappy struggle now, alas! consuming the resources and the population of a magnificent country, Mitchel returned to the Army, and, at the age of fifty-two, after a brief illness of four days, died of fever at Beaufort, being at that time a Major-General and Commander of the Federal Army in South Carolina.

Proceedings at Various Observatories.

Royal Observatory, Greenwich.

In the ordinary operations of the Royal Observatory, Greenwich, no changes of importance have been introduced during the past year; the observations have been of the usual character, and the reductions have not been allowed to fall behind.

The Star Observations from 1854 to 1860 are being incorporated into a Catalogue: this will contain about 2020 stars; star constants, precessions, and secular variations have been recomputed for all the stars. The work is now far advanced and will shortly be ready for the press.

The difference in longitude between the Royal Observatory, Greenwich, and Knightstown, Island of Valentia, has been redetermined. The very unfavourable weather of the past season protracted this operation much beyond the expected limits, and it has pressed heavily on the Observatory; the result obtained is, however, satisfactory; it agrees almost identically with the chronometrical determination of 1844. Advantage has been taken of the favourable circumstances connected with the late opposition of *Mars;* differences in Right Ascension between *Mars*, and stars east and west of the meridian, have been obtained with the Great Equatoreal. These observations are now under reduction, and it is hoped that the result may be laid before the Society at its next Meeting. *Mars* has been well observed on the meridian for combination with the Declination observations of the Southern Observatories. The method for determining the motion of the Solar System in space, proposed by the Astronomer Royal in 1859, has been applied to 1167 stars. The reductions are completed, and the results will shortly be laid before the Society.

The apparatus arranged by the Astronomer Royal for observing the fixed lines of Star spectra, and referring them to the fixed lines of the Sun spectrum, has been completed; the

arrangement is quite successful: pressure of other work has, however, prevented any systematic employment of this apparatus up to the present time.

Royal Observatory, Edinburgh.

From the Royal Observatory, Edinburgh, we hear of the usual round of official duties performed. The first and most prominent of these consists of the double system of time-signals, the visible and the audible, which are kept up daily from the two chief hills of the city, under the electric control of the Observatory and the interested surveillance of the citizens, but with a decided bias in favour of the "gun" over the "ball." Both methods have, however, at the present moment received a sudden check from the indirect effects of a most anomalous storm of lightning, which broke over the city with almost a hurricane of west wind and sleet on the 4th of February, at 7 P.M. The lightning, which is described as vividly blue and green, was seen to dart more horizontally than vertically, and at last set fire to the lower part of Nelson's Monument, an important intermediary in supporting the Observatory electric wires to both the time-ball and time-gun. The striking of a tall building so low down as in this case, or one-seventh from the bottom, is evidently noteworthy, together with the intensity of the discharge, which pierced holes of three quarters of an inch in diameter through a leaden roof, streaming at the time with conducting water, and furnished with metal pipes; but the most immediately important matter in regard to the time-signals has proved to be, that every member of one whole bundle of so-called "permanent" magnets in the controlled clock at the Castle has had its poles changed; and some similar bars in a clock at the Observatory has had two or three pairs of poles put into them.

Another, and a more considerable employment of the time and labour of the Observatory, is that required for the reduction of the meteorological observations taken by observers of the Meteorological Society of Scotland at fifty-five different stations in the country; and furnished, partly every month and partly every quarter, to the Registrar-General of Births, Deaths, and Marriages, in whose returns they are duly printed.

The Board of Visitors of the Observatory having desired that the original observations of the Teneriffe experiment should appear,—the printed volume of the *Edinburgh Astronomical Observations* has been detained for the insertion of a Teneriffe Appendix, which is now reported complete within a page or two. The same Board has also made some attempts during the past year to free the position of the Edinburgh Astronomer from the very untoward anomaly of being a public office without any salary attached to it, the actual condition in

which recent reforms at the College have left that of the Astronomers-Royal of Scotland. But up to the present time the Board has not succeeded in making any advance either in this point, or in that which they took up last year, viz., securing for the Astronomer a residence in or near the Observatory, instead of at the impracticable distance where it is now found.

Royal Observatory, Cape of Good Hope.

True to the chief object of its foundation, the Royal Observatory at the Cape of Good Hope has been occupied during the past summer by the parallax observations on *Mars*. These would have been made without doubt, whether any particular series of corresponding ones in Europe had been announced or not; and, in fact, they were made even in face of the statement contained in our *Monthly Notice* of May, which arrived at the Cape on the 15th of July, to the effect that the series of *Mars*-opposition observations prepared by a great Northern Observatory, would only be attempted if a promise of co-operation from a Southern Observatory should arrive on or before the 20th of August; and Sir Thomas Maclear knew only too well, that, though he answered by return mail, his letter could not reach Europe sooner than the 30th or 31st of August.

The Cape observations, nevertheless, began punctually on the 20th, and were continued on from that date through the whole opposition period, up to November 3d, weather permitting, with the result of measures being obtained on 45 nights out of 75. These measures were taken on the meridian with the fine transit circle of iron; others had been attempted on Mr. Airy's suggestion for the parallax in Right Ascension, with the equatoreal; but these were completely defeated by the badness of atmospheric definition when the planet was at a very low altitude, for there was nothing indicative of a "limb" to be seen about it. "I exceedingly regret the failure," writes Sir T. Maclear; "partly because the method was recommended by Mr. Airy, and partly because the advantage of the method is undeniable if a clear sky and good definition could be commanded at low altitudes. Unfortunately, atmospheric phenomena decline to be at the beck and call of practical astronomy." A most true remark, we may observe, unless the subject be treated in the manner adopted by Mahomet and the mountain; and by precisely the same act, too, viz., going to a mountain, and ascending there so high, that both observer and telescope shall find themselves in what Sir Isaac Newton describes as the serene and quiet (and necessarily rarified) air, above the grosser clouds, and which as he long ago assured astronomers is "the only remedy by which that *confusion of rays which arises from the tremours of the atmosphere*," can ever be removed from large telescopes.

The Radcliffe Observatory, Oxford.

No material change has occurred in the organisation of the Radcliffe Observatory, Oxford, since the publication of the last Annual Report of the Society. The staff of Assistants is precisely the same as at that time, and the subjects of observation have been those indicated in that Report,—namely, for Meridian observations, stars between the fifth and seventh magnitudes, and the Sun, the Moon, and the large planets, including a good series of observations of *Mars* near opposition. In addition, *Mars* has been well measured with the heliometer, and the results have been given to the Society in a Memoir by Mr. Main. *Saturn* has also been measured during the disappearance of the ring, and the resulting ellipticity agrees very closely with the former value deduced at Greenwich. The Great Comet of 1862 was observed on several evenings, and the results have been communicated to the Society. The number of double stars which have been observed with the heliometer now exceeds two hundred; but the past year has been singularly unfavourable to all classes of delicate observations.

The Carrington Transit-circle has been used throughout the year 1862 without interruption, and the results will, it is hoped, sustain the high character which it had while in the hands of Mr. Carrington.

The Photographic Meteorology has been kept up without intermission, and, in the hands of Mr. Lucas, the photographs are much better and the interruptions of continuity less than in former years.

The reductions of the observations are in a tolerably satisfactory state. The twentieth volume of the Radcliffe Series, containing the observations for 1859 and 1860, was published in July last, and the twenty-first volume is now being printed with great rapidity, the reductions of the astronomical observations for 1861 being nearly completed. The reductions of the observations for 1862 are also in progress.

Considering the smallness of the force employed, consisting of the Director, two Assistants, and one Computer, the amount of work which has been accomplished during the preceding year is highly satisfactory; and, without any further encroachment on the resources of the Trustees, a few years of such continuous labour must produce results of very great importance to astronomy.

The Cambridge Observatory.

The meridian observations made at this Observatory, under the direction of Prof. Adams, during the past year comprise, in addition to those of the fundamental stars, Observations of the Stars which were used for comparison with Comet II., 1861,

Observations of the Sun near the summer and winter Solstices and the autumnal Equinox, Observations of the planets *Jupiter* and *Neptune*, and Circle Observations of *Mars*, and of the stars selected by Dr. Winnecke to be compared with the Planet in Declination, with a view to the determination of its parallax.

The Equatoreal Observations comprise a series of observations of Comet II., 1862, and a series of comparisons in R.A. between *Mars* and neighbouring stars, taken on both sides of the meridian according to the plan described by the Astronomer Royal in the *Monthly Notices*, vol. xviii. p. 277. By this method the parallax of *Mars* may be determined by means of observations taken at a single Observatory. Charts drawn on a large scale were prepared of all the stars near the path of *Mars*, which are contained in Dr. Argelander's valuable Survey of the Northern Heavens, and the stars of comparison were selected from these. The weather was in general very unfavourable during the course of these observations, so that not many sets of corresponding evening and morning observations could be obtained.

Some new arrangements have been made in connexion with some of the instruments. The old lever counterpoises of the Transit Instrument were removed some years since by Professor Challis, and replaced by spring counterpoises, in order to allow the use of micrometer microscopes, adjusted for viewing dots placed on the ends of the pivots, so as to afford the means of eliminating any errors arising from the form of the pivots. The spring counterpoises, however, have been found to act in a very unsatisfactory manner, as the pressures which they exert are continually varying with changes of temperature. Prof. Adams has accordingly removed them and restored the lever counterpoises, introducing, however, such an alteration into their arrangement, that the apparatus for examining the forms of the pivots may now be used while the counterpoises are in action.

The pendulum of the Transit Clock, which was in an unsatisfactory state, has been put into good order by Mr. Frodsham.

In consequence of the varying length and weight of the declination rods of the Northumberland Equatoreal, corresponding to different North Polar Distances, it is impossible to balance the instrument by any system of permanent counterpoises. A series of adjustible weights has therefore been introduced, which may be very conveniently applied, and by means of which, for any given value of North Polar Distance, the instrument may be very nearly balanced in all positions. Since these weights have been in use, the performance of the driving clock has been far more satisfactory than it was before.

The Jones Equatoreal has been dismounted for the purpose of being examined. Several of the springs which relieve the pressure on the lower pivot were found to be broken, and

the microscopes were out of order. The instrument is now in the course of being put into working order by Mr. Simms.

The dome of this Equatoreal was only moved, with difficulty, by the united force of two persons, and thus the use of the instrument was rendered exceedingly inconvenient. This dome is supported on fifteen cast iron wheels, which run in channels, above and below, also of cast iron. These wheels were formerly in bearing along their whole breadth, which is considerable, and thus a large amount of sliding friction was called into play when the dome was moved. The wheels have now been turned so that the breadth of the bearing surface is very much reduced, and the result is that the dome can be easily moved by one person. Surrounding the dome is some ornamental work, supported by a rim of cast iron, which was formerly embedded in the stone wall. Of course the alternate expansion and contraction of the iron from heat and cold, split the stone-work, and thus allowed damp to be admitted into the wall; an arrangement is now made which permits the free expansion and contraction of the iron rim, and which also effectually secures the wall against damp.

Some progress has been made in the arrangement and classification of the detached Memoirs and Pamphlets belonging to the Library of the Observatory, with a view to their being bound up in volumes. Professor Challis is proceeding with the printing of Vol. XX. of the Observations, which will include the remainder of the Meridian Observations to the end of 1860.

Liverpool Observatory.

At the Liverpool Observatory, Mr. Hartnup has been recently engaged in discussing the sea-rates of nearly two thousand chronometers, for the purpose of ascertaining the average errors for voyages of different lengths. On comparing the rates at sea with the rates of the same chronometers obtained at the Observatory, the importance of attending to the thermal correction has manifested itself more strongly than ever; and, in addition to this, the rates are often found to increase or diminish with such regularity from one month to another, that a correction for this may also be very frequently applied with great advantage. Mr. Hartnup has, therefore, been induced to make considerable alterations in his method of testing and giving the rates of chronometers, with the view of ascertaining the value of these corrections more accurately, and of rendering the application of them more simple.

Glasgow Observatory.

At the Glasgow Observatory Professor Grant continues to employ the Meridian-circle in the observation of a selection of stars, chiefly below the fifth magnitude. Much of his time

during the past year has been occupied with proceedings having for their object to increase the instrumental efficiency of the establishment. The want of an instrument for making extra-meridional observations has been much felt, but this defect is in the course of being remedied by the erection of the equatoreally-mounted refractor of nine inches aperture, which belonged to the late Sir William Keith Murray, of Ochtertyre, and has been recently purchased by the University. For this valuable accession to the resources of the Observatory, the University is indebted to a few gentlemen chiefly residing in Glasgow, who have most liberally supplied the funds, not only for the purchase of the instrument, but also for the building of a separate edifice for its reception. It is expected that the instrument will be in complete working order before the close of this month.

Mr. De La Rue's Observatory.

The Cranford Observatory during the past year has been devoted mainly to photographing the Sun's disk, by means of the Kew heliograph, which has been placed there for that purpose. Mr. De La Rue's assistant, Mr. Reynolds, has obtained during the period from Feb. 7th, 1862, to Feb. 7th, 1863, about 160 photographs, one at least having been taken on every day when the state of the weather permitted. Thus a series, as continuous as possible, and extending over a whole year, has been procured, and the pictures will hereafter be measured by means of the micrometer, and the resulting data carefully discussed.

Besides photographs of the Sun, several fine lunar photographs have been taken of phases not included among the negatives previously obtained; and experiments in enlarging some of the lunar photographs to the dimensions of Messrs. Beer and Mädler's map have resulted in the production of charts of great utility and beauty. Messrs. Smith, Beck, and Beck, contemplate publishing these large charts in their entirety, for the use of selenographers; and also portions of them, suitable for the special study of those who may wish to devote themselves to the delineation of the minuter details, with the aid of eye-observations.

By the combined efforts of amateurs it is to be expected that the surface of the Moon will, in this way, be depicted with an accuracy and minuteness hitherto unattainable.

It will be recollected that Professor Selwyn, of Ely, who also has been working with an instrument constructed on the model of the Kew heliograph, but of smaller dimensions, has procured sun-pictures, on two of which, namely, on those procured respectively on Aug. 4th and Oct. 1st, he observes a notch in the Sun's limb corresponding in position with certain large spots; while eye-observations revealed a streak of the Sun's limb exterior to the supposed gap. This streak of the Sun's

photosphere is depicted on Mr. De La Rue's photographs, and the photographic and eye-observations are thus shown to be in accordance.

During the past year Mr. De La Rue's paper On the Total Solar Eclipse of 1860 has been published in the *Philosophical Transactions* of the Royal Society, and the applicability of astronomical photography to the obtaining of numerical data may be considered as having been therein clearly established.

Hartwell Observatory.

Little can be said respecting the proceedings at the Hartwell Observatory during the last year. Since the unexpected departure of Mr. Pogson, the regular work has been discontinued; but it is to be hoped that the Catalogue of Variable Stars commenced at Hartwell, and now in progress at Madras, will with time be completed.

Meanwhile, there is what may be termed a continuous task to be carried on at the Observatory by the Assistant, Mr. Samuel Horton, under the direction of Admiral Smyth,—namely, the examining and remeasuring of 145 Double Stars, selected from the Cycle of Celestial Objects.

Hopewell Observatory, Haddenham.

Mr. Dawes, owing to a combination of bad weather and bad health, has been able to make but few astronomical observations; but among them are some of especial interest. The transit of the shadow of the brightest satellite of *Saturn*, *Titan*, across the disk of the planet, has been observed; and to this may be added one observation of an eclipse of *Titan* itself in the planet's shadow. The former is a very rare phenomenon, and Mr. Dawes considers the latter to be unique. Sketches also have been made of the physical appearances of *Mars*, of Tuttle's Comet, and of certain solar spots.

It will be remembered that during the course of the present year, the late Captain Jacob, under the auspices of our Society, left England, with a telescope of adequate dimensions and of great excellence, for the purpose of observing some of the heavenly bodies at a highly favourable station in the Bombay Presidency. It is with very deep regret that the Council record the termination of this expedition and the disappointment of their well-founded hopes, caused by the untimely death of the able observer at the very moment of the commencement of his arduous work. The cost of the expedition has been defrayed partly from funds provided by the liberality of the British Government, and partly from the private resources of the lamented astronomer himself.

The Progress of Astronomy.

The Council are much gratified in being able to report that astronomical science during the past year has advanced with a steady and interesting progress. In the first place they invite the attention of the Society to the publication by Mr. De La Rue of a group of solar spots, printed in an ordinary press, from a metallic block, procured by the sole agencies of light and electricity, and untouched by any tool. This is the first instance of the application of Mr. Paul Pretsch's phototype process to the delineation of any celestial object. A second example of this important process has been given by Mr. De La Rue in his recently published Bakerian Lecture on the great Solar Eclipse of 1860. It is obvious that great will be the gain, if by the aid of this process, or by any other means, it shall ultimately become possible to multiply reliable copies of the delineations of celestial phenomena, from which have been eliminated uncontrollable errors, arising either from the imagination of the observer or from the imperfect art of the engraver.

But the successful application of photography has not been confined solely to the delineation of the physical features of celestial objects; in the hands of Mr. De La Rue it has vied in accuracy of measurement with the micrometer as ordinarily employed. As one instance of this out of many others, it may be mentioned, that at the middle of the totality of the great Solar Eclipse of 1860, the moon's diameter as obtained from the photograph was 1986″·2, while that inferred from calculation was 1986″·1.

In the next place Professor Bond's Monograph on Donati's Comet is entitled to especial commendation. Owing to the great pains, expense, and skill bestowed upon the numerous and elaborate engravings, this memoir can scarcely fail henceforth to raise the standard of excellence in astronomical illustration. A copy of Mr. Bond's work has been presented to the Society, and will probably be esteemed by the members as one of the most valuable presents of the year.

The hypothesis by which Bessel first attempted to explain the cause of the irregularities in the Proper Motions of *Sirius* and *Procyon* has received confirmation in an important degree from the recent researches of Mr. Safford of Harvard College and Mr. Auwers of Königsberg. It now appears established as an astronomical fact, that the heretofore anomalous motions of *Sirius* and *Procyon*, both in Right Ascension and in Declination, are explained and satisfied by the existence of invisible masses, round which these stars may be conceived to revolve, *Sirius* in fifty, and *Procyon* in forty years. The nature and amount of the substantial advance thus made in a most interesting but difficult branch of stellar astronomy, may be appreciated from the following philosophical remarks of Pro-

fessor Bond, to which both the Astronomer Royal and Sir John Herschel have added the expression of their general concurrence. "It is conceivable that the outstanding discrepancies from the hypothesis of a uniform motion in either Right Ascension or Declination, each series being taken by itself, might follow a systematic law which has yet nothing whatever to do with an actual change in the motion of the star, being merely an expression of the relations between errors of the determined places; and in fact Bessel's hypothesis of an invisible companion has always been open to suspicion on this ground. Now, however, the evidence excludes this suspicion, since it is in the highest degree improbable that the same law of error should hold for the discrepancies in Declination and for those in Right Ascension as well. All reasonable objection is thus removed, and the presence of an invisible mass in the neighbourhood of *Sirius*, comparable in magnitude with it, is entitled to be received as an established fact in astronomy."

In connexion with this subject it is worthy of remark that, as in the case of *Neptune*, the place of a visible celestial object was first indicated by the results of calculation; so, in the case of *Sirius* and *Procyon*, the presence of invisible masses is first felt through the agency of telescopic observation.

Among other subjects which demand our notice are two inequalities in the periodic time of Halley's Comet, discovered by M. Angström of Upsal; what is most observable in relation to this discovery is the fact of its having been deduced from data which were within the reach of all astronomers. An abstract of this short memoir appeared in our January number; a mere inspection of the plate appended to the original paper in the *Upsal Transactions*, will be sufficient for the appreciation of M. Angström's method, and of the conclusion which he derives from its application.

Practical astronomers will no doubt watch with interest the development of the properties of the new metallic alloy named Aluminium Bronze, under the hope that it may be found to possess some properties which render it a valuable substitute for gun-metal or brass in the construction of astronomical instruments. Colonel Strange has laid the results of his careful and able study of this compound so recently before the Society, that much further remark is here unnecessary. It may, however, be a matter of interest to add that in the framing of the large theodolites for the Indian Survey, Messrs. Simms have constructed some of the smaller portion with this alloy; with a laudable caution they await the results of further experience in the casting of this new substance, before they employ it to a more considerable extent.

The Society, and indeed all persons interested in the progress of physical science, will be glad to hear that the Astronomer Royal has completed an apparatus wherewith, in conjunction with the great Equatoreal at Greenwich, it is intended

to observe the dark lines in the spectra of stars. Some of these spectra have already been carefully observed and recorded by Donati; but owing to the brilliant speculations and remarkable experiments of Messrs. Kirschoff and Bunsen, there are at this moment so many questions of surpassing interest, connecting these lines with the nature of light in general, and with the physical constitution of the Sun in particular, that it is a matter for congratulation to know that the subject is also taken up by a philosopher so peculiarly competent for its investigation.

The Society will learn with much interest that Mr. De La Rue has enlarged some new photographs of the surface of the Moon in her various phases, to precisely the same scale as that adopted in Messrs. Beer and Mädler's map. It is needless to dilate on the peculiar value of such reliable delineations.

Glass mirrors of very considerable dimensions, and silvered by a chemical process, are now manufactured in Paris by Messrs. Secretan, and in Munich by Professor Steinheil, at a comparatively moderate cost. The advantages, however, which arise from the material employed, are to some extent counterbalanced by the necessity of a renewal of the silvering more frequently than a metallic speculum requires to be repolished.

Mr. Carrington has completed his Memoir containing observations during seven years and a half of the Solar Spots, and has taken preliminary steps for securing its publication. The amount of illustration indispensable to the subject has proved an impediment to its appearing in the *Transactions* of a Society whose issue is large; and it is probable that it will ultimately be undertaken by a publisher as an independent work on a subsidy of a certain amount being guaranteed. The Memoir consists of two parts, text and plates. The text contains, firstly, a preface describing fully the processes of observation and reduction, and the theory of certain small corrections not taken into account, and is followed by the auxiliary tables used at Redhill. The second section consists of a catalogue of all the nuclei which were selected as primary points of observation, arranged in columns, which need not be detailed on this occasion. The third section consists of a discussion of the deduced heliographical position of those nuclei in each group which, by aid of the diagrams, are identified as comparable on different days, and especially with the view of deducing the diurnal motions of the nuclei in both elements, which result from the use of very approximate elements of the rotation. In the next section these residual motions are assembled in a table arranged in order of the latitudes of the nuclei discussed, and the law which holds good in the mean is deduced with full evidence of the process pursued. The conclusion arrived at has already been stated to the Society at the Meeting in June last. The fifth section treats of the corrections of the assumed position of the solar equator, the theory and data being fully given. The re-

mainder of the text need not be particularly described. The plates are about 170 in number; two containing the figures necessary for explaining the method of observation and reduction; then ninety-nine plates, each showing the groups recorded during one whole rotation; then three, in which the distribution in latitude is rendered self-evident by compression; and, lastly, a series of plates necessary to supplement the rotation-plates, on which one aspect of a group can alone be shown. On these latter every appearance of a group is given, ranged in a series of squares from the top to the bottom of the plate, and thus the record is rendered complete, condensed as far as possible, and immediately instructive to the eye. No disks are given. During the past summer, Dr. Sabler, of Wilna, visited this country, to inform himself of the process of photographic registration organised by Mr. De La Rue, and was willingly allowed to copy the tables and extract all such portions of the last-mentioned Memoir, which he desired to have for use at the Observatory which the Russian Government has devoted to this branch of astronomy.

Some addition has been made to our more accurate acquaintance with the Moon's surface by the zealous and minute observations of Mr. Birt, who, during several years, has devoted much of his attention to this branch of astronomy. Mr. Birt is now examining the regions around *Plato*, under the hope of extending the discoveries made by Beer and Mädler.

Captain Shea continues with much perseverance to observe the number and position of such solar spots as are within the limits of vision of a small telescope. His delineation of the Sun's disk would be incomparably more valuable if they contained a fiducial line, or some definite co-ordinates of reference. Captain Shea's observations have now extended over a period of fifteen years, and four volumes of the results have been presented to the Society.

Planet Mars.

The planet *Mars*, during the last opposition, has been closely scrutinized by several astronomers, with instruments well adapted to such observations. The general result of these observations tends to show that the principal details visible on the surface of the planet remain constant during a long series of years. The snow or ice regions in the vicinity of the poles, however, are found to undergo changes not attributable to the effects of perspective arising from the changed position of *Mars'* axis in relation to the observer. Our energetic member, Mr. Lockyer, has succeeded in making a series of beautiful drawings of *Mars*, as seen in his 6-inch refractor, by Cook. Professor Phillips, of Oxford, has also made a

similar series, with the aid of an instrument of the same dimensions; and Father Secchi, of Rome, has also produced a very extensive series, so that materials now exist for the production of a very accurate globe of this planet.

Minor Planets.

Since the last Anniversary the number of Minor Planets has been increased from seventy-one to seventy-seven. In the middle of February it was announced by Professor Bond, that Mr. Safford, upon discussing a series of observations by Dr. Peters, presumed to belong to *Maia*, had found that only three of them could be reconciled with the observations of that planet at Cambridge, U.S., and had shown Dr. Peters' observations to apply to a previously unknown member of this group: it has since received the name *Feronia*. The *seventy-third* minor planet was detected by Mr. Tuttle, at Harvard College Observatory, U.S., on April 7, and has been named *Clytie*; the *seventy-fourth*, called *Galatea* on the proposition of Professor Littröw, was discovered by Mr. Tempel, at Marseilles, on August 29; the *seventy-fifth* by Dr. Peters, at the observatory of Hamilton College, N.Y., on September 22, and is yet unnamed; the *seventy-sixth* by Professor d'Arrest, at Copenhagen, on October 21, for which he has proposed the name *Freia*; and the *seventy-seventh* by Dr. Peters, on Nov. 12. A planet was detected by Dr. Luther, at the Observatory of Bilk, near Dusseldorf, on August 31, and has since proved to be *Daphne*, which had not been found at several previous oppositions, and indeed may be said to have been nearly lost.

Comets.

The Cometary discoveries of the year are *four* in number; the *first* Comet was found by Mr. Schmidt, at the Observatory of Athens on the evening of July 2, and two hours later by Mr. Tempel, at Marseilles; a parabola suffices for the representation of the observations. The *second* appears to have been seen at Marathon, U. S., by an amateur astronomer Mr. Swift, as early as July 15; three days afterwards it was found by Mr. Tuttle, at Harvard College, Massachusetts, and on the 22d was perceived at the Observatory of the Museum at Florence, by Mr. Toussant: other independent European discoveries have been recorded. This Comet was for some time a conspicuous object to the naked eye, and exhibited many interesting physical phenomena. It was observed in Europe till near the end of September. Observations taken by Mr. Tebbutt and others in Australia have also been published. The orbit is certainly elliptical and according to

a careful discussion by Mr. Oppolzer of the Vienna Observatory, it would appear that the time of revolution is about 142 years. Computations by other astronomers sufficiently confirm this result. The *third* Comet was detected, on the morning of November 28, by Professor Respighi, at Bologna, and three days later by Dr. Bruhns, at Leipsic. It is still within reach of powerful instruments. The orbit does not sensibly differ from a parabola, so far as can be judged from calculations up to this time. The *fourth* Comet, discovered by Dr. Bruhns on the morning of December 2, was not observed in Europe after the middle of the month, on account of its rapid motion towards the south. A fortnight's observations are well satisfied by a parabolic orbit.

The short-period Comet of Brorsen has passed its perihelion unobserved, chiefly owing, no doubt, to its unfavourable position in the morning twilight.

The Council in their Report of the astronomical proceedings of the year cannot but advert to the recent perilous balloon ascents of their colleague, Mr. Glaisher, to heights above the earth's surface heretofore unreached. One of the main objects of these ascents was to extend and improve our knowledge of the relation which exist between increase of elevation and the corresponding variations of temperature and moisture, these variations in their turn having an intimate bearing on the theoretic determination of atmospheric refraction.

The results of Mr. Glaisher's observations indicate that the hypothesis of a diminution of 1° Fahr. of temperature for every additional 300 feet of elevation must be abandoned, even for inconsiderable heights above the earth's surface. The variation of temperature for the same variation of altitude is found also greatly to depend on the clearness or cloudiness of the sky. Thus, through the first 1000 feet, under a cloudy sky there is a change of 1° Fahr. for every 213 feet, but if the sky be clear there is a diminution of 1° Fahr. for every 139 feet. At extreme heights, such as between 25,000 and 30,000 feet, it requires a full 1000 feet of additional altitude to cause a diminution of temperature of 1° Fahr.

In conclusion, the Council have to report that among the subjects which have engaged their attention during the year, the promotion of the Survey of the Southern Hemisphere, on the plan of Dr. Argelander, has had special consideration. A committee, consisting of the Astronomer Royal, Mr. Hind, and Mr. Carrington, was appointed to confer on the subject, and finally handed in the following report at the first meeting of this year, which the Council have unanimously adopted, and ordered to be printed as part of the Annual Report of this day.

"1. The proposal which was submitted to the Council in the month of June was principally based on the expressed wish of Dr. Argelander that a Survey of the Southern Hemisphere of Stars should be undertaken by British means on a similar scale to that which he had in hand at Bonn; and the proposition has been renewed, of late years, frequently in his private correspondence, as he was approaching the termination of his undertaking. The Committee fully agree with his feeling that it is highly desirable, for the progress of Astronomy, that such a Survey of the Southern half of the Heavens should be made, and that the ideal of the two halves of the Survey, being contemporaneous, should not be departed from more than may be unavoidable. The Committee believe that the promotion of undertakings such as the present lies fully within the scope of action of the Society, as defined by its By-laws; and they think that the Council may usefully take into consideration, at the present time, whether advantage may be taken of one of the stations which the country possesses in the Southern Hemisphere—as at the Cape of Good Hope, Sydney, Williamstown, Melbourne, or Hobarton.

"2. The work involved in the proposed Survey is of very simple character, but of considerable extent. The number of stars to be observed may be estimated at about 300,000, each to be twice recorded, without reckoning the revision of imperfect zones, and searches for the purpose of removing doubtful cases. By the employment of four persons simultaneously, the work of observation, reduction, formation of standard catalogue, collation with previous authorities, and formation of maps, would, as for the Northern half, under equally efficient superintendence, necessarily require the labour of from six to eight years—an estimate which, admitting of little correction, forbids the entertainment of the supposition that there is any probability of the task being at any time executed by private enterprise. The work is essentially one which must be performed by the staff of an official observatory, or by some special addition to the fixed staff of an establishment; and this being the case, the support of a body such as the Royal Astronomical Society, and the assistance of external funds, appear the only available means of carrying out a work of the kind contemplated.

"3. Should the Council take this view of the matter, the Committee are prepared to state that the requirements, as respects instrumental means, are so small as to constitute no difficulty whatever. The selection of an Astronomer to whom the superintendence of the undertaking could confidently be intrusted is felt by the Committee to be the first position to be secured, in order to ensure success; but they are not as yet empowered to make any recommendation in this respect.

Further than this no difficulty is foreseen with respect to suitable assistants in the execution of the work, inasmuch as the details call for nothing beyond intelligence, method, and the degree of zeal which may be expected to come of itself to a task so directly competitive with a similar undertaking. Many young men may with ease be found who would readily and efficiently take their part in such a work, for such inducement of pay and position as would ordinarily be offered in such a case; and though the annual amount of salaries would be larger than the Society has hitherto applied to Government to provide at their suggestion, the total is but little in comparison with what has been granted in past years for other scientific purposes. There is the great advantage, in this instance, that the work is perfectly defined by the parallel Survey in the North, and that the return for the outlay may be regarded as certain beforehand.

"4. Lastly, the Committee have to mention that the Director of the Madras Observatory has notified his intention of executing a portion of this work forthwith at his station, which, apart from the accidents of life and health in an unhealthy place, may be expected to somewhat reduce the extent of the area to be surveyed independently of him. But it will be borne in mind that Madras is 13° N. of the Equator, and that this notice from Mr. Pogson can at most slightly modify the general proposal before the Council.

"5. The Committee have agreed to report thus generally, in the first instance, on the state of the case, and on the questions involved in the proposal which might otherwise be thought to entail difficulties with which it is in fact not surrounded, and abstain from making special recommendations till the inclination of the Council to move in the matter has been ascertained."

The Council, which this day retires, have thus endeavoured by preliminary inquiry to clear the ground for action in this matter on the part of their successors in office, to whom they commend the further promotion of the undertaking.

Papers read before the Society from February 1862 *to February* 1863.

1862.

Mar. 14. Discovery and Observations of Comet III. 1861. Mr. Bond.

On the Perturbations of *Uranus* and Mass of *Jupiter*. Mr. Safford.

On the Proper Motion of *Sirius* in Declination. Mr. Safford.

Moon Culminators, Greenwich, Sydney, and Cape of Good Hope. Mr. Scott.
Observations of Comet II. 1861. Mr. Mackay.
On the Irregularity of the Proper Motion of *Sirius*. M. Auwers.
Observations at Malta: Discovery of New Star in Trapezium of *Orion*. Mr. Lassell.
Transit of *Mercury*, &c. Mr. Abbott.
Ephemeris of Long-Period Variable Stars for 1862. Mr. Pogson.
On some Variable Stars. Mr. Knott.
Observations of Transit of *Mercury* and Solar Eclipse. M. Bulard.
On Astronomical Refraction. Mr. Yates.
Occultation observed at Highbury. Mr. Burr.
Orbit of ξ *Ursæ Majoris*. Mr. Breen.
Observations of Solar Eclipse, Dec. 31, 1861. M. D'Arcy.

Apr. 12. On Solar Spots. M. Jeanjaquet.
Rediscovery of *Alhazen*, a supposed Lunar Crater. Mr. Birt.
Opposition of *Mars*, &c. Lieut. Gilliss.
Observations of Encke's Comet. Mr. Hartnup.
On the Disappearance of a Nebula in *Coma Berenices*. Sir J. Herschel.
Note of a Circular Dark Spot on the Sun's Disk. Mr. Lummis.
Transit of *Mercury*. Mr. Abbott.

May 9. Observation of a Meteor, April 14, 1862. Mr. Gorton.
Transit of the Shadow of *Titan* across the Disk of *Saturn*. Rev. W. R. Dawes.
On Heliotypography. Mr. De La Rue.
On Hind's Nebula in *Taurus*. M. O. Struve.
Results of Meridional Observations of Small Planets, March 1862. Mr. Airy.
On the Asteroid *Feronia*. Dr. Peters.
Observations at Cambridge of Comet II. 1861. Mr. Adams.
Extract of Letter to Mr. Carrington on the Intra-Mercurial Planet. Mr. Littrow.
Extract of Letter to the Astronomer Royal on the Transit of *Mercury*. Mr. Todd.
On Encke's Comet: Extract of Letter to the Astronomer Royal. Sir T. Maclear.
On Lambert's Theorem for Elliptic Motion. Mr. Cayley.
On a Solar Spot and on a Transit of *Titan* over *Saturn*. Mr. Gorton.
Observation of the Appearance of the Ansæ of *Saturn*. Mr. Carpenter.

On the Variable Star U *Geminorum*. Mr. Baxendell.
Transit of the Shadow of *Titan*. Mr. Lockyer.
Observations of Comet I. 1861. Sir T. Maclear.
Announcement of New Instrument for Indian Trigonometrical Survey. Col. Strange.

June 13. Results of Meridional Observations of Small Planets. Mr. Airy.
On the Appearance of *Saturn's* Ring. Mr. Birt.
On the Solar Energy in the Autumn of 1861 and Spring of 1862. Mr. Birt.
Occultation: Disappearance of *Jupiter's* 1st Satellite. Mr. Talmage.
Extract of Letter from Dr. Winnecke. Rev. R. Main.
Disappearance of B.A.C. 4923 at the Moon's Dark Limb, 1862, June 9. Rev. R. Main.
Letter to the Astronomer Royal. M. Hoeck.
Extract of Letter to the Astronomer Royal. Mr. Scott.
On the Missing Nebula in *Coma Berenices*. M. Chacornac.
On some Phenomena attending the Disappearance of *Saturn's* Ring, May 19, 1862. Mr. Huggins.
On the Periodical Changes in the Belts and Surface of *Jupiter*. Mr. Huggins.
Saturnian Phenomena. Rev. W. R. Dawes.
On the Appearance of *Jupiter* without a Visible Satellite, Sept. 1843. Rev. W. R. Dawes.
On the Proposed Parallax. Observations of *Mars*, 1862. Prof. Smyth.
On the Solution of Kepler's Problem. M. De Gasparis.
Description of a New Aplanatic Eye-piece for Telescopes. Mr. Burr.
On the Mean Places of certain Fixed Stars. M. C. Rümker.
On a Solar Spot. Mr. Hodgson.

Nov. 14. On the Cluster *κ Crucis*. Mr. Abbott.
Observations of the Horizontal Refraction of the Sun. Major Lang.
On the Irregularities of the Motion of *Procyon*. Mr. Lynn.
On the Practicability of Observing the Occultation of Stars by the Moon at Sea. Mr. Smith.
Extract of a Letter from Dr. Wolfers. Rev. R. Main.
Measures of the Planet *Mars*. Rev. R. Main.
Observations of Comet II. 1862. Mr. Knott.
Ditto ditto Mr. Hartnup.
Ditto ditto Mr. Chalmers.
Ditto ditto Lieut. Chimmo.
Observations of a Comet. Mr. Abbott.
On the Extraordinary Brightness of the Lunar Spot *Aristarchus*. Dr. Stothardt.

On a Remarkable Chain of Lunar Craters. Mr. Birt.
On a New Method of obtaining the Value of the Divisions of a Transit Level. Capt. Noble.
On Combining the Angles of Regular Polyhædrons. Mr. Drach.
On Excentric and Centric Force. Mr. Pratt.
On the Identity of the Triple Star H. I. 13. Rev. W. R. Dawes.
Second Series of Observations of Double Stars at Madras. Mr. Powell.
On the Motion of a Pendulum with especial reference to the Foucaultian Experiment. Mr. Wackerbarth.
On Aluminium Bronze. Col. Strange.
Telescopic Appearance of *Mars*. Mr. Grove.
Letter to Mr. De La Rue on the True Figure and Dimensions of the Earth. M. Von Gumpach.
Account of Observations with the Elchies Equatoreal. Prof. C. P. Smyth.
Observations of Comet II. 1862. Rev. R. Main.

Dec. 12. On the Variable Star R *Vulpeculæ*. Mr. Knott.
A last Letter from Capt. Jacob. Prof. C. P. Smyth.
Letter presenting Helioautographs. Rev. Prof. Selwyn.
Observations on *Saturn*. Mr. Wray.
A New and Close Approximation to the Circumference of the Circle, &c. Mr. Drach.
On the Gregorian Calendar. Mr. Drach.
Observations of a Comet. Mr. Cockburn.
On the Form of Lenses for Negative Eye-piece of Telescopes. Mr. Airy.
On the Great Solar Spot of August 4, 1862. Mr. Howlett.

1863.
Jan. 9. Extract of Letter to the Astronomer Royal. M. Auwers.
Letter, with Observations of *Saturn* made at Pulkowa and Greenwich. Mr. Airy.
Observations of *Mars*. Mr. Joynson.
Observations of a Comet in Australia. Mr. Tebbutt.
Observations of *Asia* (67). Mr. Hartnup.
Observations of Comet II. 1862. M. Romberg.
Results of Meridional Observations of Small Planets. Mr. Airy.
On the Observations made with the Elchies Equatoreal. Rev. W. R. Dawes.
Elements and Ephemeris of *Feronia* (71). Dr. Peters.
On the Elchies Equatoreal. Mr. Knott.
Note on U *Geminorum*. Mr. Baxendell.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
The Secretary of State for War.
The Secretary of State for India.
The Lords Commissioners of the Admiralty.
The Board of Trade.
The Postmaster General.
Royal Society of London.
Royal Asiatic Society.
Royal Geographical Society.
Royal Institution.
Royal United Service Institution.
Geological Society.
Linnean Society.
Zoological Society.
Photographic Society.
Society of Arts.
British Meteorological Society.
British Association.
Art-Union of London.
Institute of Actuaries.
British Horological Institute.
Radcliffe Trustees.
Syndicate of Cambridge Observatory.
Lancashire Historic Society.
Liverpool Mercantile Marine Association.
Literary and Philosophical Society at Manchester.
Royal Society of Edinburgh.
Scottish Society of Arts.
Royal Irish Academy.
Royal Dublin Society.
Royal Society of Van Dieman's Land.
Imperial Observatory, Paris.
Imperial Observatory, Vienna.
Royal Observatory, Munich.
Royal Observatory, Brussels.
Royal Observatory, Madrid.
Observatory, San Fernando.
Observatory at Bonn.
L'Académie Impériale des Sciences de l'Institut de France.
Le Dépôt Général de la Marine.
Le Bureau des Longitudes.
L'Académie Impériale de Dijon.
The Society at Cherbourg.
Imperial Academy of Sciences, Vienna.
Royal Academy of Sciences, Berlin.

The Society of Physics, Berlin.
Royal Academy of Sciences, Munich.
Royal Academy of Sciences, Brussels.
Royal Academy of Sciences, Göttingen.
Royal Academy of Sciences, Amsterdam.
Royal Society, Naples.
Royal Swedish Academy.
Imperial Academy, St. Petersburg.
Hungarian Academy.
Academy of Sciences, Batavia.
Society at Berne.
Society at Upsala.
Society at Geneva.
Society at Leipsig.
Austrian Board of Marine.
Canadian Institute.
Government of the United States.
Committee of Harvard College.
Observatory of Harvard College.
American Academy of Arts and Sciences.
American Philosophical Society.
Smithsonian Institution.
Franklin Institute.
Committee of the American Coast Survey.
Boston (U.S.) Library Trustees.
Editors of Silliman's Journal.
Editor of the Athenæum.
Editor of the Literary Gazette.
Editor of the Parthenon.
Editor of the Critic.
Editor of the London Review.
Editor of the Reader.
Editor of the Intellectual Observer.
Editor of the Sculptor's Journal.

G. B. Airy, Esq.
M. Vallas Antal.
Prof. Argelander.
Prof. Armellini.
Dr. A. Bache.
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J. J. Waterston, Esq.
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Messrs. Willis and Sotheran.
M. Winnecke.
Dr. R. Wolf.
H. Worms, Esq.
J. Yates, Esq.
Dr. A. Ziegler.

Address delivered by the President, Dr. Lee, on presenting the Gold Medal of the Society to M. Argelander.

Gentlemen,—The established custom of this Society enjoins me to undertake the important duty of addressing you on the termination of our year of office, and of resigning into your hands the trust which you confided to me. But my gratification on this occasion is mixed with regret, inasmuch as this is the last time that I shall have the honour of speaking officially to you as President of the Royal Astronomical Society. The first idea which now occurs to me, is that of requesting you to accept of my respectful and grateful thanks for the honour which you have conferred upon me in placing me in this chair,—an honour which will be the source of pleasing reflections during the remainder of my days; and, whilst feeling that only a zeal for your interests, and in a life-long desire to promote the advancement of our noble Science, I can pretend to vie with those eminent men who have preceded me in this Presidency.

The opportunities of great discoveries in the regions of space, or in the theory of the grand and simple laws which rule the motions of those vast bodies which decorate the heavens, have not entirely passed away. The times of Kepler and of Newton, of Lagrange and of Bradley, will not again return. Where they reaped the rich harvest of astronomical truths we can but glean. We must rest content with the investigation of what has been well termed, by Sir John Herschel, "Residual Phenomena."

The principal Observatories of Europe, and of Africa, and of Asia, and of America, are now engaged in the gradual accumulation of systematic observations, and in recording facts, which, hereafter reduced to order, will serve as bases of verification of the accepted theories of our time, or will themselves become the germs of new theories, or the elements of undiscovered laws among the Astronomers of future ages.

Within the comparatively short period of the existence of this Society, of which Sir William Herschel was the first President in 1820, and of which, two of the Council, and two of the three original Secretaries are still existing, and shining as stars of the first magnitude in their diverse regions of Science, and all of them being Members of the Royal Society, what a world of splendid incidents, bearing on its pursuits, have been revealed to skill and perseverance!

Such are, The improved knowledge of Cometary Motion; the investigation of the Nebular Theory; the repetition of the Cavendish Experiment; the "Resisting Medium" discussion; the taking of Transits by Galvanism; the Chronoscopic intercourse between Observatories; the Electric Telegraph, and its application to the longitudes of places; the Undulatory Movement of Light; the application of the new and surprising art of Photography to the Sun, the Moon, and even the double stars; the detected affinity between the Solar Spots and Terrestrial Magnetism; the orbits of Binary Stars; the lines of the Solar Spectrum; the triumphant winning of Neptune, and the finding of upwards of seventy Minor Planets between *Mars* and *Jupiter*.

Amongst those things deserving of record in the progress of celestial observations within the last two years, it would be unpardonable in me, were I to pass over in silence the dauntless excursions in the firmament undertaken by our indefatigable colleague, Mr. Glaisher, and his fearless companion, Mr. Coxwell. At one time much was expected to accrue to Meteorological Science from aeronautic expeditions; but I think it may be safely conceded, that from the days of Gay Lussac and Biot to our own, little has been done deserving of scientific record until the important discoveries which these two gentlemen have brought to our knowledge by their successful balloon ascents during the preceding year.

Such are the great and interesting results extracted from

the recesses of nature, by skill and perseverance, in the brief span of time to which I have alluded; and it is gratifying to recollect the practical aid which some of our Members have rendered towards the advancement of these invaluable branches of Science, as well as the Society's liberal recognition of all the labourers in this vineyard, whatever clime or country they pertained to. This is well exemplified by noting the awards to merit made from year to year by our successive Councils; and in this, as you are all aware, it has been a principle well understood and acted upon in the adjudications, that the mere official product of public establishments, however admirably executed, is not to be placed in competition with those spontaneous investigations which spring from the voluntary ardour of individual exertions, unless improved methods are introduced.

In illustration of this, I may remark that our exponents of high approbation of astronomical labours are seen in the bestowal of our honorary distinctions.

Since the founding of this Society in 1820, there have been forty-three medals awarded to merit, of which nineteen have been bestowed on foreigners, and twenty-four on our own countrymen, a very fair proof of the cosmopolite principles on which we have hitherto acted.

I now come to the subject which is the principal object of this Address, namely, the Council's award of the Gold Medal for this year to F. W. A. Argelander, the distinguished Professor of Astronomy at the University of Bonn, in Prussia, —a gentleman whose high claims will occur to you all, on a recollection of his able superintendence of the several observatories of Abo, Helsingfors, and Bonn, the various works which he has published, and the numerous communications which he has made to the scientific public in Baron de Zach's *Correspondance Astronomique*, and the *Astronomische Nachrichten*. The Observatory of Bonn was founded by the laudable and intelligent spirit of the Kingdom of Prussia, and the whole of the expenses of the establishment are supported by the well-bestowed liberality of that country.

A large portion of the Professor's researches has been devoted to that crucial element, the Proper Motions of the Stars. The general method of ascertaining these has been by comparing two sidereal Catalogues with a considerable interval between their epochs, thereby to detect any difference which does not correspond with the amount of the precession of the equinoxes; yet, as the earlier catalogue is more liable to the consequences of instrumental errors than the more delicate observations of recent days, the real truth can only be extracted from repeated operations and improved methods. Numbers of supposed erratic bodies are cleared off the working list at each succeeding course of investigation; but those which are thereby confirmed and proved, become of the gravest importance for

all future investigations of the Stellar Laws. To this problem Argelander applied his experience and skill with such success, that in the year 1835 he published his well-known Catalogue of 560 Stars, with their motions as determined by his observations in Finland. Under this examination he assumed, that of the 560 stars, 390 of them had proper motions exceeding 0″·1 per annum; and from this he deduced a very interesting result relative to the movement of the Solar System towards a point in the heavens, presenting a near agreement with that which was roughly conjectured fifty-two years before, by our peerless extra-meridian Præses, the late Sir William Herschel.

By the refined investigation of the Abo results, the co-ordinates of the point in question, referred to the mean equinox of 1792, are

R.A. =	259° 47′·6	with a probable error of	3″·28
Dec. = +	32° 29′·5	do.	2″·20

The course of these studies led the Professor to estimate the comparative magnitudes of the stars by the unassisted eye, and he arrived at the conclusion, that in the space between the North Pole and 30 degrees of Southern Declination, there are 3256 stars visible to the naked eye.

Many faint stars had been included in former celestial charts, while brighter ones had been neglected. Argelander found about twenty stars which, though visible to the naked eye, were not contained in any trustworthy Catalogue. The importance of thus giving to future astronomers the means of judging whether any gradual and very slow changes are taking place in the relative magnitudes is sufficiently evident. He also watched assiduously for the changes attributed to those singular bodies, the Variable Stars. Hence followed his remarkable disquisition from his own personal observations, on the double maximum and minimum curve of *β Lyræ*, in 1843; a result which, though differing in particulars, confirmed the researches of our countryman (Goodricke) in 1784; to whom is due the discovery of the changes of brightness in *β Lyræ*. This memoir, perhaps, stimulated the attention of modern observers upon the strange phenomena which these stars exhibit. This course he has followed out with signal success; and in our *Monthly Notices* for June 1856, we have copied from the *Astronomische Nachrichten* his able suggestions respecting a "Method for determining the Brightness of the Minor Planets."

In the same *Notices* for December 1861 is a Report on the progress of the admirable Star-Charts in course of execution at Bonn. It is communicated by our indefatigable colleague, Mr. Carrington, who received it from Dr. Krüger, by the direction of Professor Argelander.

This is a singularly interesting Report. It describes the various parts of the magnificent Catalogue, which will embody

30,000 (accurately) determined stars. It is, therefore, well deserving of a reperusal by all our Members, as it explains lucidly what portions of the Stellar Zones are finished and fully published. Also, the part which was in the press, and what share remained in the press at that time, are duly detailed; and it must be a matter of gratification to an English reader, that the Catalogues of our Members,— Groombridge, Johnson, and Carrington,— have been closely consulted in the construction of this great product of science; and they are associated with the eminent foreigners, Lalande, Piazzi, Bessel, Struve, Schwerd, and Rümker.

The remark at the conclusion of this communication is deserving of our full attention, and, I think, approbation; namely, "That the Fellows of this Society, and all to whom "this account comes, cannot fail to admire the stately progress "of this enormous work, which, simple in its conception, and "free from insurmountable difficulties in execution, would "*appal* many an astronomer by its vast extent."

The Zone Observations of Dr. Argelander, contained in the first two volumes of his work, and made at Bonn, are of great importance. This extensive and valuable work may be looked upon as a continuation of Bessel's Zone Observations. These latter were limited by the parallels of 15° South and 45° North Declination. The Zones contained in Dr. Argelander's first volume extend from 45° to 80° of North Declination, and those in the second volume from 15° to 31° of South Declination. As in Bessel's Zones, the observations of each Zone are accompanied by an Auxiliary Table for reducing the apparent places of the stars observed, to the mean places for a given epoch. For the Zones in the first volume, the epoch chosen is the beginning of 1842, and for the second the beginning of 1850. The former Zones contain 26,424 separate observations, of which about 4000 are duplicate, so that the number of different stars contained in them is about 22,000. The latter, or Southern Zones, contain 23,250 observations, of which about 5600 are duplicate; so that the number of separate stars is about 17,600.

Thus we have a total of nearly 40,000 stars, whose places have been determined with very considerable accuracy, the probable error of a determination of Right Ascension corresponding to an equatoreal interval scarcely exceeding $0^s.1$, and that of a determination of Declination scarcely exceeding 1″, except for some of the more Southern Zones.

Bessel's Zones comprise about 75,000 observations of nearly 62,000 different stars. Mr. Carrington's excellent Redhill Catalogue contains the places of nearly 4000 stars between the Pole and 81° of North Declination, the observations being made on a somewhat more extended plan than those of Bessel and Argelander.

Thus, by the junction of the labours of these three astronomers, the places of above 105,000 stars lying between the

North Pole and 31° of South Declination have been well determined. It would be highly desirable that Zone observations on a similar plan to that of Bessel and Argelander, should be undertaken by some zealous astronomer in the Southern Hemisphere, so that the survey might be extended to the South Pole.

Vast as is the number of stars whose places have thus been determined, it is far exceeded by the number recorded in Dr. Argelander's Survey of the Northern Heavens, which forms the basis of his beautiful new Star-Charts, and for which your Council have awarded him the Medal of the Society.

The first section of this work, forming the third volume of the *Bonn Observations*, contains the approximate places of 110,984 stars between 2° South and 20° North Declination. The second section, forming the fourth volume, contains the approximate places of 105,075 stars between 20° and 40° of north declination.

Dr. Argelander's plan of survey extends to the whole of the Northern heavens; but, in the hope that a similar survey may be undertaken in the Southern hemisphere, he includes the first two degrees of Southern declination, for the sake of affording means of comparison between the two works.

Within the above-mentioned limits the Charts were to contain all stars down to the 9th magnitude, as far as possible all the brighter ones of the 9-10 magnitude, and as many of the remainder of this class as circumstances permitted. The magnitudes were not to be more than a half-class in error, and the positions in both co-ordinates not more than one minute in error, so that every star in the Charts might be at once found again in meridian instruments; and that, with respect to neighbouring stars, no doubt should occur which was meant. All gross errors in existing Star-Catalogues were also to be noticed and corrected, whether they related to the magnitudes or positions of the stars.

As an instance of the utility of this work of Dr. Argelander, it may be mentioned that this survey immediately supplied proper stars to be compared with *Mars*, with the view of determining his parallax by equatoreal observations during the last autumn. Also, it enables us at once to identify stars that have been compared with planets and comets. Also, to detect and correct any errors of entire minutes, and such-like errors, which not unfrequently occur in recorded observations or in Catalogues.

It is said that Professor Challis has made great use of Dr. Argelander's last published Catalogue in identifying stars observed with the transit and mural circle of the Observatory of Cambridge, and in correcting or verifying the minutes of right ascension and north polar distance given by the observations. In cases of discordances between the results of observation and the places of a working catalogue of zodiacal stars,

founded on the British Association Catalogue, and those of Weisse and Lalande, he was generally able to determine on which side the error lay, by means of Argelander's Catalogue; and the large number of such instances afforded good evidence of its trustworthiness and accuracy.

Argelander began his Zones in the spring of 1852, and finished them in the spring of 1859.

The number of stars to be registered is, in all, about 310,000, as follows:—

0°	to	20°	105,000
20	to	40	93,000
40	to	60	69,000
60	to	80	37,000
80	to	90	5,000
			309,000

Such have been the laborious and persevering exertions of our experienced Associate to render the knowledge of our European skies as perfect as, humanly speaking, is possible by his admirable Zones in continuation of Bessel's, his beautiful Sidereal Charts, and his unequalled Survey of the heavens of the Northern Hemisphere, which comprises all the first nine magnitudes of the stars; and for which your Council have awarded Professor Argelander the highest honorary distinction which it is in their power to bestow.

My anticipation, therefore, is, that the Members will, by an unanimous vote, confirm this decision of their Council; which Council I can affirm, in all its consultations, has freely devoted the best of its judgment to this result. Indeed it has willingly employed its time to the promotion of the interests of science, and of your reputation, in all its operations during my occupation of this Chair.

The President then addressed the Foreign Secretary thus:—

Admiral Manners,—In transmitting this Medal to Professor Argelander, as an acknowledgment by this Society of his zeal, perseverance, ability, and industry, in sidereal investigation, be good enough, with your usual courtesy and kindness, also to convey to him, with our best wishes for his health and happiness, the deep interest which we have taken in his past, and which we must continue to take in his future, labours for the promotion of Astronomy.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

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G. B. AIRY, Esq. M.A. F.R.S. Astronomer Royal.

Vice-Presidents:

ARTHUR CAYLEY, Esq. M.A. F.R.S.
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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIII. *March* 13, 1863. No. 5.

G. B. AIRY, Esq., Astronomer Royal, in the Chair.

On the Fringes of Light observed by M. Poulain at Goree during the Total Eclipse, 31st December, 1861. By M. Hermann Goldschmidt.

(*Extract from a Letter addressed to the Astronomer Royal.*)

I have seen in the *Monthly Notices* (which the Royal Astronomical Society kindly send me) the interesting drawing you laid before the Society, representing the fringes of light observed by M. Poulain at the total eclipse observed at Goree on the 31st Dec. 1861.

Allow me, Sir, to give you some details on the same object, concerning the conversation with the Spanish countryman who journeyed for some hours in our company in going from Vitoria to Bilbao. He stated that before the totality of the Eclipse of 1860, seen by him on a high mountain, yellow spots were seen flitting over their white clothes from West to East. He compared the colour with the most yellow flowers (like the *Ibericum*) we saw on the road. There is, indeed, no useful scientific information in this relation, but it confirms the direction of the movement observed by myself on the annular Eclipse of the year 1820, and mentioned by me in the descrip-

tion of the Solar Eclipse of the year 1860 (see *Astron. Nachrichten*, No. 1340, p. 308). It may be of some interest to give you the whole description of what I had seen in Frankfort-on-the-Maine on the afternoon of the 7th September, 1820.

I was in my uncle's house, one hundred and twenty paces only from my father's, when the darkness began. I left the house; but I had scarcely gone twenty paces in the street, from East to West exactly, when I saw the moving shadows coming towards me, covering the large and deserted street, and passing under my feet. Their movement was slow, for otherwise I could not have perceived them so distinctly, with the eyes so nearly fixed on them; the movement was not rapid. The aspect was like the shadow of smoke in sunshine—regular in translation; the forms, like rhomboids of four or six inches in diameter, and sometimes more extensive, were mixed with superposed shadows of the same size, and ribbon-like forms. The inner spaces particularly, filled with round spots softly melted and mixed with the whole in veiled grey transparence, gave to the phenomenon a somewhat mysterious appearance. Thus, looking to the paved street, these ribbons and spots flitted under my feet, till I arrived at a group of men near my father's house, after having made one hundred steps in contemplating this strange apparition. At this moment the annular eclipse was forming: the light of the Sun ran around the Moon in a very singular manner—it seemed like a fluid mass. I can truly estimate the time from the moment when I saw the shadows till the forming of the annulus—an interval of two minutes and thirty seconds.

I spread a large white cloth at my feet in Vitoria, but I had no time to look on it, absorbed by other business and the people who rushed in.

I had recommended this observation to Captain Manheim, who really saw the fringes at Constantine in the year 1860.

Paris, February 2nd, 1863.

On the Determination of the Longitude of Valencia, in Ireland, by Galvanic Signals, in the Summer of 1862. By G. B. Airy, Esq., Astronomer Royal. (Abstract.)

The Astronomer Royal adverts to the operation effected in 1844 for the determination of the longitude of the Feagh Main Station, in Valencia, by the transmission, ten times in both directions, of thirty pocket-chronometers, between Greenwich and Feagh Main; a transit instrument having been erected at Feagh Main, and the chronometers being compared with the transit-clocks at Greenwich and Feagh Main. Advantage was taken of the passage through Liverpool and

Kingston to determine the longitudes of the Liverpool Observatory and of a station near Kingston Harbour. The longitudes then found were

		m s
Liverpool Observatory, West of Greenwich		12 0·05
Kingstown Station	,,	24 31·20
Feagh Main Station	,,	41 23·23

For details, he refers to *Mem. Roy. Ast. Soc.*, vol. xvi., or *Greenwich Observations*, 1845.

After the completion of galvanic communication between London and Valencia (consequent on the laying down of the Atlantic Telegraph), and the connexion, through the agency of the London District Telegraph and the British and Irish Magnetic Telegraph, between Greenwich and the London Offices of the B. and I. M. Telegraph Company, the Astronomer Royal, with the assured assistance of Sir Charles T. Bright and E. B. Bright, Esq. (Engineer and Managing Officer of that Company), and of T. B. Moseley, Esq., (their London Resident Officer), decided on repeating the determination by galvanic signals. The principal motive for this was, the desire of co-operating with M. Struve in establishing an Arc of Parallel from Valencia in the West to Orsk, on the river Oural, in the East. The station now adopted was not that of Feagh Main, but a place in Knightstown, in the vicinity of the Telegraph Office.

Sir Henry James and Captain Clarke, R.E., gave great assistance in the loan of observing hut, the conveyance of instruments, and other ways; and the Knight of Kerry and R. J. Leckie, Esq., contributed all the local aid that was required.

It was determined that local time at Valencia should be found entirely by observation of zenith distances, and a 12-inch Altazimuth (by Simms) was lent for this purpose by T. Coventry, Esq. The signals were simply movements of a galvanometer-needle at each station, the current being given at every 15ˢ nearly by an auxiliary clock.

The observers, Mr. Dunkin and Mr. Criswick, remained at Valencia from June 11 to July 23. During this time there were few opportunities of star-observation in consequence of the continued bad weather, and few opportunities of observing galvanic signals in consequence of interruptions at the Telegraph Office of Mallow. On the whole, only six nights of effective signals with good determinations of local time were obtained. The Valencia observer throughout was Mr. Dunkin, and the Greenwich observer of signals Mr. Ellis.

On reducing the altazimuth observations, great anomalies presented themselves in the determination both of latitude and of time. After carefully discussing the observations, and

making some experiments at Greenwich, it became clear that the zenith-distances were always given too great by about 4″. The cause of this anomaly is not known. On applying the corresponding correction, the results were made harmonious. It is remarked, however, that very little depended on this correction, the observations having been well balanced on the East and West sides of the meridian.

The signal-observations were then discussed nearly in the same way as in the determination of the longitude of Brussels (*Mem. Roy. Ast. Soc.*, vol. xxiv.), and the results were the following:—

	m	s
Time occupied in the passage of the current	0	0·129
Longitude of the Knightstown Station	41	9·81

By the kindness of Captain Clarke a geodetic connexion was made between the Feagh Main Station and the Knightstown Station, and their geodetic difference of longitude was found to be $13^s{\cdot}56$. Applying this to the former determination for Feagh Main, it is found that the old operations would have given for Knightstown $41^m\ 9^s{\cdot}67$. The agreement of this with that found from the galvanic operation is very remarkable, and it proves that implicit confidence may be placed in the old determinations for Liverpool and Knightstown.

On the Movement of the Solar System in Space, deduced from the Proper Motions of 1167 *Stars.* By Edwin Dunkin, Esq., of the Royal Observatory, Greenwich.—(Abstract.)

The investigation which forms the subject of this Memoir, may be considered as a supplement or addendum to that of the Astronomer Royal, *On the Movement of the Solar System in Space*, published in vol. xxviii. of the Society's *Memoirs*. The method of calculation differs in no respect from that previously used, excepting that the Proper Motions of a much greater number of stars have been combined in producing the results. It is intended, therefore, that this paper should be read in connexion with the memoir of Mr. Airy, as the mathematical explanation, which has been given in detail by him, is not generally repeated on this occasion.

In this memoir, the Proper Motions of nearly all the stars contained in the Catalogues of the Rev. R. Main, have been adopted, numbering in all 1167 stars. In Right Ascension they are scattered in nearly equal proportions over the twenty-four

hours, and in Declination, about two-thirds are situated in the northern hemisphere. The stars are arranged into groups or divisions, according to magnitude, in conformity with the researches of W. Struve, whose value of r has been adopted. The number of stars included in each division, and Struve's value of r, are as follows:—

Division.	Magnitude.	Value of r.	Number of stars in each Division.
1	1	1·00	9
2	1,2 2 2,3	1·71	55
3	3,2 3 3,4	2·57	146
4	4,3 4 4,5	3·76	238
5	5,4 5 5,6	5·44	330
6	6,5 6 6,7	7·86	368
7	7,6 7 7,8 8	11·34	21

The method which the Astronomer Royal has proposed for determining the direction of solar motion, as being entirely free from the objections attaching to those of former investigations, "and, indeed, unobjectionable on the score of accuracy, consists merely in removing the primary geometrical notions from the apparent movements of the bodies in space. It is the obvious and simple method of treating the linear movements of the Sun, and of each star, by the use of rectangular co-ordinates. This method is in itself absolutely independent and complete, and requires no assumption of a point determined by preceding investigation."

For convenience of reference, the final symbolical equations from which the different numerical equations are immediately derived on two extreme suppositions, are inserted in the memoir. For the first supposition, it is assumed that the irregularities of Proper Motion are due to chance-error of observation; while in the second supposition they are considered to be entirely due to a peculiar motion of the stars.

The numerical equations for X, Y, Z, have been formed for each division on both suppositions, $-X$ representing the annual linear motion of the Sun in the direction of the first point of *Aries*, $-Y$ the motion in the plane of the equator towards that point whose right ascension is ninety degrees, and $-Z$ the motion towards the north pole of the equator. The solution of the sums of these equations gives the following values, neglecting very small terms depending on n' and q.

On the first supposition:

$$X = +0{\cdot}0428 + 1{\cdot}2349 \times n'$$
$$Y = +0{\cdot}2776$$
$$Z = -0{\cdot}1818 + 3{\cdot}5792 \times q$$

On the second supposition:

$$X = +0{\cdot}0406 + 2{\cdot}3747 \times n'$$
$$Y = +0{\cdot}3696$$
$$Z = -0{\cdot}1735 + 6{\cdot}1090 \times q.$$

From the above the following expressions are deduced:—

On the first supposition:

$$R = 0{\cdot}3346 \quad + 0{\cdot}1580 \times n' - 1{\cdot}945 \times q$$
$$A = 261^\circ\ 14'\ 0 - 4{\cdot}3454 \times n'$$
$$= 261\ \ 14\ \ 0 - 248^\circ{\cdot}97 \times n'$$
$$D = +32\ \ 55\ \ 0 - 0{\cdot}3057 \times n' - 8{\cdot}9811 \times q$$
$$= +32\ \ 55\ \ 0 - 17^\circ{\cdot}52 \times n' - 524^\circ{\cdot}54 \times q$$

On the second supposition:

$$R = 0{\cdot}4103 \quad + 0{\cdot}2350 \times n' - 2{\cdot}5826 \times q$$
$$A = 263^\circ\ 43'\ 9 - 6{\cdot}3480 \times n'$$
$$= 263\ \ 43\ \ 9 - 363^\circ{\cdot}71 \times n'$$
$$D = +25\ \ 0\ \ 5 - 0{\cdot}2672 \times n' - 13{\cdot}4937 \times q$$
$$= +25\ \ 0\ \ 5 - 15^\circ{\cdot}31' \times n' - 773^\circ{\cdot}13 \times q.$$

The preceding results have all been reduced, on the assumption that the distance of the stars varies inversely to their magnitude, regardless of the amount of Proper Motion. It is by no means improbable, however, that the few stars of the fourth, fifth, and sixth magnitudes, whose Proper Motions are extravagantly large, are really near stars, and as such might be placed with propriety in the first division. The results of a transfer of this kind, in addition to others of a similar nature, are given in the memoir. A glance at the values of X, Y, Z, determined by these different assumptions, shows distinctly that they are not relatively improved by any deviation from Struve's law of the mean distances of the different groups of stars with respect to these exceptional stars.

The value of R, or the Sun's proper motion as viewed from a star of the first magnitude, expressed in seconds, resulting from this investigation, differs very slightly from that determined by M. Otto Struve. His value is 0·3392. There is very little doubt, therefore, that a value near the mean of these numbers may be assumed as fairly representing the annual motion of the Sun and solar system towards the point to which the direction is assigned, so far as any result can be obtained by the use of the known apparent proper motions of the stars.

To show the numerical evidence of the probability that the values previously given should in some measure represent the motion of the solar system, the sums of the squares of the observed Proper Motions in Parallel and North Polar Distance

were formed, and these have been compared with the squares of the residual errors after the corrections depending on solar motion have been applied. This comparison was only made on the second supposition. The results are exhibited in the memoir for each division; the total sums of the squares are as follows:—

Sum of squares of Motion in Parallel	Uncorrected	= 78·7583
	Corrected	= 75·5831
Sum of squares of Motion in N.P.D.	Uncorrected	= 63·2668
	Corrected	= 60·9084

"The reader must draw his own conclusions on the comparison of the uncorrected and corrected results given above. It appears to me that our fundamental suppositions rest altogether on a very slender basis; and the results warrant the question whether we have much ground to infer that the Proper Motions of the stars are produced principally by the motion of the Sun and its system in space. It may be, and probably such an inference is partially true, that these apparent motions in the positions of the stars are due to some compound effect resulting from causes some of which have yet to be discovered; though it is very remarkable that different astronomers, with totally different methods of investigation, should, without exception, fix the point in the heavens to which the solar motion is directed, within a few degrees of each other."

The memoir concludes with a comparison of the position of this point as determined by different astronomers, from the date of Sir William Herschel's original paper in 1783, to the present time. The author remarks on the extraordinary agreement of these researches:—"To say the least, this confirmation of result after result, would seem to show that the Proper Motions of the stars *must* be connected with the motion of the Sun in the direction of the point in the heavens indicated above. But the further consideration of this I must leave for others, merely remarking, that the grounds upon which we have been working are uncertain to a considerable extent, which uncertainty will scarcely be removed until our knowledge of the distances of the stars is increased, or until the Proper Motions themselves are re-determined from unexceptionable observations, made at both epochs, with the improved instruments of modern Observatories."

A table is appended to the memoir, containing the principal elements from which the numerical co-efficients have been computed. The stars are arranged in their respective divisions, in order of Right Ascension, excepting that all stars of North Declination are grouped together, the same being also done for stars of South Declination.

On the Rating of Chronometers. By J. Hartnup, Esq.

At the Liverpool Observatory, Mr. Hartnup has been recently engaged in discussing the sea-rates of nearly two thousand chronometers, for the purpose of ascertaining the average errors for voyages of different lengths. On comparing the rates at sea with the rates of the same chronometers obtained at the Observatory, the importance of attending to the thermal correction has manifested itself more strongly than ever; and, in addition to this, the rates are often found to increase or diminish with such regularity from one month to another, that a correction for this may also be very frequently applied with great advantage. Mr. Hartnup has, therefore, been induced to make considerable alterations in his method of testing and giving the rates of chronometers, with the view of ascertaining the value of these corrections more accurately, and of rendering the application of them more simple.

The following is an example:—

From the week ending November 1, 1862, to the week ending January 24, 1863, the temperatures to which the chronometer was exposed were 55°, 70°, 85°, 70°, 55°, and so on in succession; it, therefore, requires five weeks for a complete series of rates in the above-named temperatures, and these were obtained between October 25 and November 29, the observed rates being as follows:—

	Temp.	Rate.
	°	s
Nov. 1	55	+0·9
8	70	+0·4
15	85	−0·7
22	70	+1·6
29	55	+2·1

Now the rates in 55° for the week ending November 1 being $0^s{\cdot}9$ gaining, and in the same temperature for the week ending November 29 being $2^s{\cdot}1$ gaining, the difference is $1^s{\cdot}2$, which, divided by 4, the number of intervening weeks, gives $0^s{\cdot}3$ for the weekly increase of gaining rate.

We, therefore, have for November 15:—

	Mean daily rate	losing $0^s{\cdot}7$ in 85°
From Nov. 8 and 22	,,	gaining 1·0 in 70
From Nov. 1 and 29	,,	gaining 1·5 in 55

And weekly acceleration of gaining rate 0·3

With the above data the error on Greenwich Mean Time has been calculated for each week subsequent to November 15, by adding $0^s{\cdot}3$ × by the number of weeks to that mean daily

rate which corresponds with the temperature to which the chronometer was exposed, multiplying the sum by 7, and adding the amount thus obtained to the error for the preceding week. That is, Calculated Daily Rate = Daily Rate Nov. 15 for same temperature + $0^s{\cdot}3$ × number of weeks since Nov. 15. For example, for the week ending Jan. 17, temperature 70°, Calculated Daily Rate = $1^s{\cdot}0 + 9 \times 0^s{\cdot}3 = 3^s{\cdot}7$, which, multiplied by 7, is = $25^s{\cdot}9$, and Calculated Error for Jan. 17 = Calculated Error for Jan. 10 + $25^s{\cdot}9 = 0^h\ 3^m\ 58^s{\cdot}1 + 25^s{\cdot}9 = 0^h\ 4^m\ 24^s{\cdot}1$.

In this way the following Calculated Errors in Greenwich Mean Time have been obtained, and compared with those found by observation, from November 22, 1862, to January 24, 1863, and the Observed — Calculated Errors range from + $3^s{\cdot}8$ to — $8^s{\cdot}5$ for the eight weeks subsequent to the last observed error and rate used in the calculation.

	Temperature to which Chronometer was exposed.	Error on G.M.T. Calculated.	Observed.	Obs^n — Cal^n.
1862.		h m s	h m s	s
Nov. 22	70	0 2 12·4	0 2 14·5	+2·1
29	55	2 27·1	2 29·4	+2·4
Dec. 6	70	2 40·4	2 42·6	+2·2
13	85	2 43·9	2 47·6	+3·7
20	70	3 1·4	3 5·2	+3·8
27	55	3 24·5	3 27·1	+2·6
1863.				
Jan. 3	70	3 46·2	3 47·5	+1·3
10	85	3 58·1	3 57·6	−0·5
17	70	4 24·0	4 18·9	−5·1
24	55	0 4 55·5	0 4 47·0	−8·5

If the corrections for temperature and acceleration of rate had not been applied and the $0^s{\cdot}7$ losing rate found for the week ending November 15 had been used, then, by exposing the chronometer to a temperature of 55° for the eight weeks, the Greenwich Mean Time by the chronometer would have been wrong nearly four minutes; and if, on the other hand, the rate for 55° had been used and the chronometer exposed for the eight weeks to 85°, the error would have been only a few seconds. In the former case the defective thermal compensation and acceleration of rate both tend to increase the error, while in the latter they nearly neutralise each other. It is, therefore, not surprising that we frequently hear of so much difference being found between the sea and land rates of chronometers; nor can it be expected but that we meet with very discordant results in attempting to find the average error to which chronometers are liable at sea. The average error for a voyage of eight weeks, found from data given at foreign ports, is a little under one minute; but while the

Greenwich Time shown by ten per cent of the chronometers from which this result is obtained, is found to be nearly free from error, there are also ten per cent in which the average error is upwards of four minutes.

On the Parallax of certain Stars. By M. Auwers.

(*Extract of a Letter to the Astronomer Royal, dated Gotha, 1863, Jan. 16.*)

Since I communicated to you the notices on the Parallaxes of two stars, I have made an examination of Johnson's observations of 61 *Cygni*, in the hope that the Oxford heliometer, if the observations made by it are treated in such a manner as to make the known systematic faults of heliometer-distances exercise the least possible influence on the results sought, may give a decisive voice for one of the two irreconcileable values for the Parallax, namely Bessel's of 0″·370, and Struve's and mine of 0″·553.

I have taken as the subject of calculation the quantity 0″·705 × dist. of 61 *Cygni* from B.A.C. 7320 — dist. of 61 *Cygni* from Lalande 41030, instead of the distances themselves, or their immediate differences; my hopes, however, have not been confirmed.

The whole series of observations gave the Parallax =0″·426; the residual errors, however, are of such a nature that we cannot look upon the observations as sufficiently represented with this value of Parallax. Still less is this the case, if, instead of using the annual variation of the above-given differences that follows from the measures themselves, we assume that which follows with greater security from the meridian observations, with which the Parallax comes out 0″·402. But if we calculate separately the observations of the eleven first months (40 in number), and those of the remaining seven months, we obtain from the former the Parallax = 0″·526, from the latter only = 0″·192; the first of these values agrees very well with Struve's and mine, whilst the second departs materially from all determinations, and seems to show that the observations made after June 1853 are affected with some systematic error.

On the Parallax of the Fixed Stars. By M. Krüger.

(*Lettre de M. Krüger à M. Le Verrier.*)

The following letter appears in the *Bulletin Météorologique*, 1863, Feb. 9th.

"Permettez moi de vous communiquer le résultat de deux séries d'observations faites à l'aide de l'excellent héliomètre de Bonn sur des parallaxes d'étoiles fixes. La première se rapporte à l'étoile Lalande 21258, 8·9eme grandeur, dont le grand mouvement propre a été signalé par M. Argelander dans le No. 1288 des *Astronomische Nachrichten*. J'ai trouvé par 36 comparaisons avec deux étoiles, dont l'une précède, l'autre suit, à peu près sur le parallèle :—

"Parallax annuelle de J. Lalande 21258 = + 0″·260 avec l'erreur probable ± 0″·02.

"La seconde recherche concerne l'étoile de 9eme grandeur, 17415–6 dans le Catalogue de M. Oeltzen, pour laquelle M. Argelander a trouvé un mouvement annuel de 1·2 secondes. Au moyen de 45 comparaisons avec deux étoiles convenablement situées, le résultat suivant a été évalué :—

"Parallax annuelle de Oeltzen 17415–6 = + 0″·247, avec l'erreur probable ± 0″·021.

"*Helsingfors*, 1863, *Janvier* 31."

Results of the Meridional Observations of Small Planets; Occultation of a Star by the Moon; and Phenomena of Jupiter's Satellites; observed at the Royal Observatory, Greenwich, in the month of January, 1863.

(*Communicated by the Astronomer Royal.*)

Victoria (12).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Jan. 24	13 15 13·8	9 30 39·00	88 23 52·01
27	13 0 43·6	9 27 56·07	88 17 0·01

Bellona (38).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Jan. 24	13 16 13·9	9 31 39·32	78 17 31·21
27	13 2 17·9	9 29 30·68	77 53 19·56

Urania (30).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Jan. 19	9 16 14·6	5 11 17·83	65 10 55·38
20	9 12 1·8	5 11 0·86	65 12 47·74
23	8 59 40·0	5 10 26·74	65 18 13·61
24	8 55 37·0	5 10 19·58	65 19 55·63
27	8 43 40·9	5 10 11·21	65 24 41·73

Ariadne (43).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Jan. 24	12 33 49·8	8 49 8·19	76 17 52·45
27	12 18 41·8	8 45 47·45	76 7 59·58

Europa (52).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Jan. 7	12 11 25·0	7 19 38·26	72 5 2·33
20	11 9 18·9	7 8 37·25	71 10 13·91
23	10 55 11·3	7 6 16·94	70 57 32·27
24	10 50 30·5	7 5 31·95	70 53 26·28
27	10 36 33·7	7 3 22·58	70 40 57·38
29	10 27 21·3	7 2 1·71	70 32 47·00

All the observations of N.P.D. have been corrected for refraction and parallax.

Occultation of a Star by the Moon.

Day of Obs.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
			h m s	
1863, Jan. 27	Disappearance	Dark	5 9 49·1	D.
27	Reappearance	Bright	6 28 30·6	D.

Phenomena of Jupiter's Satellites.

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time.	Observer.
1863.			h m s	
Jan. 24	I.	Eclipse disapp.	13 53 45·8	C.
27	II.	Eclipse disapp.	13 8 34·9	D.

The initials D. and C. are respectively those of Mr. Dunkin and Mr. Criswick.

Occultations observed at Highbury. By T. W. Burr, Esq.

1863, Feb. 27th, 15 *Geminorum* disappeared at $8^h\ 13^m\ 21^s{\cdot}4$, Local Sidereal Time. March 2d, *α Cancri* disappeared at $10^h\ 28^m\ 34^s{\cdot}5$, Local Sidereal Time.

Both occultations were by the Moon's dark limb. The star 15 *Geminorum* was very faint, and in no case could the emersion be accurately observed. Telescope as usual, my 4 ft. 4 in. Equatoreal, $3\frac{3}{8}$ inch aperture. Power 173. Longitude, 24^s W.

Highbury, March 12th, 1863.

On the Visibility of Stars in the Pleiades to the unarmed Eye. By G. B. Airy, Esq., Astronomer Royal.

To the greater number of star-gazers, with what are commonly considered "good eyes," I believe that Ovid's remark as to the visible number of *Pleiades* still applies:—

"Quæ septem dici, sex tamen esse solent."

I find, however, that one of the members of my family habitually sees seven stars, and, on rarer occasions, twelve. On the clear evening of 1863, Feb. 15th, a map of the visible stars was drawn from ocular view, and, on comparing it with

North.

a map drawn from Bessel's measures, I had no difficulty in identifying the stars as those which Bessel has marked by the following numbers:—

No. 1	No. 17 (bright)
2 (bright)	29
4	34 (brightest = η *Tauri*)
4 (bright)	38
11 (bright)	41 (bright)
13	42 (not quite so bright)

No. 42 is the seventh star, which is seen only by acute eyes.

Observations of Mars. By W. Lassell, Esq.

I have the pleasure to present to the Society herewith a series of drawings of the planet *Mars* during the late favourable opposition.*

The more deeply-shaded parts of the series are generally the blue and neutral-tint portions—the lighter shades represent the ruddy or deep-orange parts—and the unshaded parts, the brightest portions, which are generally about the circumference. The faint lines exterior to the disk represent approximately the position of the ecliptic with respect to the phenomena of its surface. The times given are Greenwich Mean Time.

The drawings are *tracings* of the original drawings made at the telescope's side. Towards the North Pole there was generally a bright part, less marked, however, than the conspicuous spot at the South Pole. This is most striking, as might be expected, in those phases in which the neighbouring portion is most shaded. On the 22d Sept. (No. 3 drawing) a bright marginal crescent, on the following side, extended nearly over half the circumference. No. 4 (24th Sept.) was drawn from a very sharp image and with great care. On the 25th Oct. (No. 15) the long band of white near the North Pole was remarkably plain. The phenomena were best seen with a Power 760. On the 27th Oct. (No. 16) the planet exhibited with 760 a very fine view of delicate markings, very difficult to draw. There seemed to be a protuberance of the limb about (*c*), where also the limb was whiter than at the parts adjacent. A long white tract was visible at (*b*) only a little inferior to the brightness of the white spot at (*d*). There was a blueish-white district at (*a*). The dark or neutral-tint portion seemed shaded off there into a lighter hue of the same

* These drawings will probably appear in lithograph in the volume of *Memoirs*.—Ed.

colour—not at all into the coppery tinge. The ruddy portion was much more uniform, as indeed it generally is, than the blue or green portion. The only distinct mark I saw upon it was a curved streak near the South Pole, which appeared to be a very slight shade of the blue colour. Several of these remarks apply generally to most of the drawings; but I make them here because the view obtained this evening was one of the very finest I have ever had. Nov. 4th (No. 17), power 760, the planet has already become obviously gibbous. The blue portion has in its darkest part a very irregular and hard outline, which is strikingly analogous to the coast-line of a terrestrial continent, much broken by bays and inlets. I find it impossible to represent its outline faithfully. There are also delicate markings on the red or brown portion near the bright spot, which baffle my skill. The view with this high power is at times surprisingly sharp.

In watching *Mars* for so long a period I expected to have been able to observe returns of the same phase according to the law of rotation, but I have not found it possible to do so except very imperfectly. To watch the progress of any individual spot during a few hours observation on any one evening, or after the interval of one day or two, is easy enough; but when the same phases ought again to occur after an interval of time, I find they can rarely be certainly recognised.

For instance, the phase given

Sept. 13,	ought	nearly	to recur	Oct. 22
,, 20	,,	,,	,,	,, 29
,, 22	,,	,,	,,	,, 31
,, 24	,,	,,		Nov. 2
,, 25	,,	,,	,,	,, 3
,, 27	,,	,,	,,	,, 5
,, 29	,,	,,	,,	,, 7
Oct. 11	,,	,,	,,	,, 19
,, 13	,,	,,	,,	,, 21
,, 15	,,	,,	,,	,, 23
,, 17	,,	,,	,,	,, 25
,, 18	,,	,,	,,	,, 26
,, 21	,,	,,	,,	,, 29

but if the drawings nearest these dates are severally compared, the resemblance is certainly traceable in most instances, but not with that uniformity which would indicate an unchanged surface. I therefore think, as has been justly remarked by Mr. Grove in the *Monthly Notice* for December, that if the variously coloured portions do generally represent land and water, their aspect must be greatly modified by the transit of clouds of great extent, density, and variety of form.

It will be seen that there is much less detail in the later drawings, which are scarcely comparable with the early ones on account of the greatly increased distance of the planet.

Proceedings of the Madras Observatory. By Norman Pogson, Esq., Director of the Observatory.

The following Memorandum, relating to the current and the proposed work of the Madras Observatory, has been transmitted by Mr. Pogson to the Astronomer Royal:

"The proceedings of the Madras Observatory during the past two years, although not yet before the public, nor indeed likely to be so at present, may not be without sufficient interest to warrant a brief recapitulation. The new Transit Circle was finally completed and commenced its career of service on the 1st June, 1862. It yields the most satisfactory results as regards steadiness in all its corrections, and is being employed for the determination of the positions of comparison stars used for equatoreal observations, of all observable variable stars, of such small planets as come to opposition south of the equator, and generally for the formation of a Catalogue of new small southern stars as zero points for the Southern Celestial Survey, now in hand. This latter work, announced in the *Monthly Notice* of last June, was commenced on the first night of this year, and will be proceeded with rapidly, aided, it is hoped, by an experienced assistant whom it is intended to request Prof. Argelander to nominate, so soon as the sanction already granted by the local government has been confirmed by the supreme authorities.

"The Variable-Star-Atlas is just beginning to be engraved, and as from its nature it can only be proceeded with on perfectly clear moonless nights, must necessarily be some time in hand—interfering certainly to some extent with the Southern Survey, but not being in any way delayed by the contemporaneous execution of that far higher and more comprehensive work now fairly in hand. A useful Catalogue of Stars observed under the management of Lieut.-Col. Worster, when acting Astronomer, is in an advanced state of reduction. Observations of the fine comets of 1858, 1861, and 1862, and of several small planets, await the determination of the comparison stars employed by the Transit Circle, all which will, it is hoped, be secured in the ensuing fine season.

"The Magnetical and Meteorological records, carried on hourly for the twenty years 1831–1860, are wearily advancing as regards reduction and publication. The former are printed up to 1854, the latter a year further on. These heavy arrears

form the most trying portion of the observatory duties, and most grievously interfere with the astronomical department at the present time. Twenty districts, each averaging about seven talugs, are now made to send rain returns to the Observatory, and the arrangement of these, more especially the sifting and summing up of the registers of eight years past, will be no trifling additional task.

"The planet *Mars* was duly observed as proposed when in opposition, for the investigation of the Solar Parallax, both with the Equatoreal at rising and setting, and with the Transit Circle at meridian passage; but owing to pressure of other immediate duties, the observations have not yet been sufficiently reduced for submission to their respective head-quarters, viz., Greenwich and Pulkova.

"Some highly desirable longitude determinations are under contemplation throughout India, at no distant period, by means of the electric telegraph, and as a first step thereto, the construction of a chronograph has been sanctioned by the Local Government. A new Equatoreal of larger aperture and a superior Anemometer are expected to be supplied shortly. The present instruments are all in fair order, having been cleaned up and repaired by Mr. F. Doderet, the mathematical instrument maker to the Public Works Department, from which his services were transferred to the Observatory throughout the first half of the past year."

Madras, 1863, *Jan.* 26.

At a recent meeting of the Royal Society, our Fellow, Mr. Huggins, in conjunction with Dr. W. A. Miller, the Treasurer of that Society, communicated a Note on the "Lines in the Spectra of some of the Fixed Stars," as an earnest of the results of twelve months' observations of some 30 or 40 stars, the Moon, *Jupiter*, and *Mars*, which are to follow.

The valuable observations of Dr. Miller on the photographic spectra of the metals, &c., are too well known to require mention here, did not a recent deduction from them hold out great promise of doing for the *temperature* of the Sun and stars what the magnificent generalisation of Kirchoff has done for their *chemical constitution*. It has been shown that the spectra of metals vary with the temperature of the source of light, the higher the temperature the more refrangible being the vibrations. Thus, by introducing lime and sulphate of magnesia into the oxyhydrogen jet, the flame of which, according to Bunsen, cannot exceed 14580° Fahr., a spectrum has been obtained corresponding in length with that of the Sun; by employing the

electric spark between magnesium terminals, however, a very decided band is obtained beyond the limits of the solar spectrum. Now, as magnesium is well known to exist in the Sun, it would thus appear that the temperature of that luminary is not so high as that of the electric spark, and that approximately it is not higher than that of the oxyhydrogen flame.

Next in importance to a form of apparatus which enables the lines to be clearly seen, must be reckoned the arrangement by which they are measured and referred to Fraunhofer's lines, the universally accepted standard of comparison. Mr. Huggins, having proved the exact coincidence of the sodium band with the solar line D on his apparatus (which is attached to his equatoreally-mounted 8-inch refractor), has employed it as the point from which all measures are taken, the sodium band being compared directly with the stellar spectrum under observation, both being in the field of view of the telescope. To guard against every source of error, however, in such delicate investigations, the spectra of one or two of the metals have also been compared with the stellar spectrum in some cases; and it is the intention of the authors of the Note to supplement their observations with these additional direct comparisons in each instance.

It may be mentioned that since this valuable communication was made to the Royal Society, Dr. Miller and Mr. Huggins have succeeded in obtaining microscopic photographs of the spectra of *Sirius* and *Capella*. Diagrams of the lines in *Sirius*, *α Orionis*, and *Aldebaran*, which had been observed and measured with great accuracy, accompanied the paper.

J. N. L.

An account, by Prof. Secchi, of his latest observations on the same subject, together with a sketch of the Spectra of *α Orionis*, *Aldebaran*, *Sirius*, *Rigel*, and *Pollux*, will be found, *Ast. Nach.*, No. 1405.—Ed.

A communication has been received from M. de Gasparis, "*Sur une Equation pour le Calcul des Orbites Planetaires.*" This has already been published in the *Ast. Nach.*, No. 1405, and in the *Comptes Rendus* of the Academy of Paris.

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*Companions of Sirius.**

M. Goldschmidt has announced to the Academy of Sciences at Paris the discovery of several stars in the neighbourhood of *Sirius*:—

"The unexpected discovery of a companion of *Sirius*, by Mr. Clark, confirms in a striking manner the existence of a celestial body in the neighbourhood of *Sirius*, theoretically predicted by Bessel, Peters, Auwers, and Safford. The companion perceived by Mr. Clark—is it illumined by *Sirius*, or a Sun shining by its own proper light? In the latter case, we should have two stars physically combined, forming a system of double stars. Having tried to see this companion with my feeble instrument of 46-lines aperture, I was able to perceive it at moments; I was, moreover, very much astonished to see that this companion was not alone, and that there were several other stars in the neighbourhood of *Sirius*, at distances varying from 15″ to 1′. The first (*c*) of these stars, or companions, is remarkable for its proximity to the central star; its position, approximately estimated, corresponds to an angle of 95°, reckoned from east to north. This new companion is sufficiently visible at moments, with the precautions necessary for such an observation; for all these minute stars, five in number, are immerged in the light of *Sirius*. The discovery was made with an object-glass of 46 lines; Mr. Clark employed an object-glass of 216 lines (18 inches). The difference of light was, therefore, as 1 to 22. The figure represents the six satellites (image reversed).

Position of the Satellites *b*, *c*, *d*, *e*, *f*, *g*, in regard to *Sirius* (*a*).

"These researches have been made independently of the remarkable theories of MM. Peters, Auwers, and Safford. Conversing with M. Radau, I was able to establish that there was a striking agreement between the data of M. Auwers and the observation of the Star No. 1 (*Ast. Nach.*, No. 1371)."

M. L'Abbé Moigno remarks that M. Goldschmidt remains faithful to the motto on the Medal of the Society, "*Quidquid nitet notandum.*"

* Translated and the woodcut extracted from No. 5 of *Les Mondes*, the weekly scientific journal edited, in place of the *Cosmos*, by M. L'Abbé Moigno.—Ed.

The following Errata in the *Monthly Notices*, together with some previously corrected, were communicated to the Editor by the Astronomer Royal.

Vol. XX., 1860.

Page 368, line 39, *for* *α Orionis*, *read* *ε Orionis*.

Vol. XXI., 1861.

Page 143, line 6, *for* outlines, *read* outliers.
— 196, — 12, *for* 1811, *read* 1861.
— 211, — 26, *for* third satellite, *read* first satellite.
— 244, — 15, *for* Best, *read* Birt.

Vol. XXII., 1862.

Page 278, line 8, *for* 18 inches, *read* 13 inches.
— 295, — 22, *for* ring of the shadow, *read* shadow of the ring.
— 322, — 14, *for* 161, *read* 164.

Vol. XXIII., 1863.

Page 70, line 12, *for* mere, *read* more.
— 75, last line but one, *for* their being, *read* with their being.
— 79, line 9, *for* 880·54, *read* 180·54.
— 92, — 26, *for* 133·6, *read* 183·6.
— 100, — 19, *for* place, *read* phase.
— 113, — 6, *for* P.E., *read* P.M.
— 120, — 14, *for* 65, *read* 98.
— 120, — 17, *for* 69, *read* 99.
— 120, — 24, *for* 117, *read* 116.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXIII. *April* 10, 1863. No. 6.

G. B. Airy, Esq., President, in the Chair.

Lieut.-Col. Patrick Stewart, R.E., 8 Lower Belgrave Street, and
J. Wm. Harris, Esq. M.R.C.S.L., Southernhay, Exeter,
were balloted for and duly elected Fellows of the Society.

Determination of the Sun's Mean Horizontal Equatoreal Parallax, from a Comparison of Corresponding Observations of N.P.D. of Mars and Stars, made at the Royal Observatory, Greenwich, and the Government Observatory, Williamstown, Victoria, Australia, during the Opposition of 1862. By E. J. Stone, Esq.

In his Tables of *Mars*, published in the *Annals of the Imperial Observatory*, Paris, 1861, M. Le Verrier remarks that it is impossible to reconcile the observations of *Mars* with theory without attributing to the perihelion a motion greater than any which can be obtained, except by a sensible increase of the received planetary masses; that the necessary agreement between theory and observation could be obtained by increasing the received value of the mass of the Earth in proportion to the Sun's mass, by not less than a tenth part, but that such an increase in the received value of the Earth's mass would necessitate a corresponding increase in the received value of the Sun's mean equatoreal horizontal parallax of a thirtieth part.

M. Le Verrier deduced the same result from a discussion of the latitudes and the motion of the node of *Venus*. The dif-

ficulties raised in the theory of *Mercury*, although not removed, were slightly diminished by the same increase of the Earth's mass.

In his Solar Tables, M. Le Verrier has adopted the value $8''\cdot95$ for the mean equatoreal horizontal solar parallax; this value was obtained by determining from observation the coefficient of the lunar equation and assuming the mean lunar parallax and data furnished by the theories of Precession and Nutation.

The way in which M. Le Verrier has thus evolved from the theories of *Venus*, the Earth, and *Mars*, the necessity of a value of the mean solar parallax much greater than the usually received value $8''\cdot57$, and not differing greatly from $8''\cdot95$, must render it extremely probable that the true value of the Sun's mean parallax does not differ greatly from that quantity.

It is the object of the present paper to lay before the Society a redetermination of the Sun's mean equatoreal horizontal parallax.

Some time since Robert L. S. Ellery, Esq., the Director of the Government Observatory, Williamstown, Victoria, Australia (Longitude E. $9^h\,39^m\,39^s\cdot3$, Latitude S. $37^\circ\,52'\,6''\cdot6$), forwarded to the Astronomer Royal a fine series of meridian declination observations of *Mars*, made during the period extending from 1862, August 20, to the middle of the following November. These observations the Astronomer Royal has kindly placed in my hands for comparison with the corresponding Greenwich observations. I have thus deduced 22 independent determinations of the Sun's mean solar parallax.

The method of reduction pursued has been as follows:—The North Polar Distances of *Mars* at the Greenwich and Williamstown transits, and the geocentric distances of the planet, have been obtained, where necessary, by interpolation from the *Nautical Almanac*. The Greenwich observations were then compared with the tables; and from the system of errors thus exhibited a curve was roughly drawn to represent the change of error: the only use of this curve was to determine the small change of error of tabular places between the transits of *Mars* over the meridians of Williamstown and Greenwich.

The factors of the Sun's mean horizontal equatoreal parallax in the expressions for the parallax in declination were then calculated, using the Astronomer Royal's value of the ratio of the principal axes of the earth, viz. $\frac{299}{300}$; and the observations of *Mars* compared with the same stars at Greenwich and Williamstown. In weighting the observations, I have considered the probable error of an observation of the centre of *Mars*, deduced from observations of the two limbs, equal to the probable error of a good star observation: the weights are the strict theoretical weights, and I have considered a

observations as equally good, unless the observer has expressed himself dissatisfied with the observation at the time of making it. All the Greenwich observations that could be combined with observations at Williamstown, without trusting to the interpolation of change of tabular error for more than two days, and without using different stars of comparison at the two stations, have been so combined, and the results are given. The following are the separate results:—

1862.	Mean Horizontal Equatoreal Parallax.	Weight.	1862.	Mean Horizontal Equatoreal Parallax.	Weight.
	″			″	
Aug. 25	9·240	3·1329	Oct. 3	9·024	2·9241
27	9·646	3·4969	8	8·962	2·8561
28	9·416	2·2201	9	9·024	2·8224
Sept. 3	8·868	1·7689	11	8·748	2·7556
10	9·161	4·2436	18	8·497	0·5329
11	8·756	4·0000	22	8·929	2·4025
16	8·844	3·7249	24	8·559	1·4884
17	9·148	2·8224	29	8·708	3·6100
21 & 22	8·684	4·3264	Nov. 3	8·620	0·4356
23	9·074	4·3681	9 & 10	8·932	2·2201
25	9·052	2·9584	12	9·006	1·5876

The mean result is 8″·932, and if we assume the probable error of a single observation = 0″·25, the probable error of the above result would be 0″·032.

The comparison of the Greenwich and Williamstown observations, therefore, leads to the value 8″·932 ± 0″·032 for the Sun's mean horizontal equatoreal parallax,—a value differing but slightly from the value assumed by M. Le Verrier in his Solar Tables.

M. Otto von Struve, Associate of the Society, gave an account of a remarkable local deviation in the direction of gravity, which has lately been observed in Russia. It is the more remarkable from its occurring in a country which is totally free from mountains, and which, though slightly undulating, may be described as a very level plain country.

On computing, upon the best elements for the figure of the earth, the longitudes and latitudes of the points in the Russian triangulation, it was found that the observed co-latitude of the Observatory of Moscow was about 8″ greater than that given by the geodetic connexion with other well-determined points of the survey. To put this beyond doubt, it was compared with numerous triangulation-stations in its neighbourhood, and the

results, with only such discordances as may always be expected, were all of the same kind. The high character of Professor Schweizer, the observer at Moscow, placed the validity of the astronomical determination beyond all suspicion. Professor Schweizer, then, with such assistance as he could command, repeated the observations at numerous points in the vicinity of Moscow. The following is the general result:

Upon the map below there will be remarked three somewhat irregular lines, nearly in the direction E.N.E. and W.S.W. Along the central line the observed co-latitudes agreed sensibly with the geodetic co-latitudes. To the north of that central line, for a considerable distance, the observed co-latitudes are too great, and to the south of that central line they are too small. The upper and lower lines show the places of maximum discordance; the upper line passes sensibly through the Observatory of Moscow. The disturbances appear to have been traced to about one degree of longitude on each side, east and west, where their magnitudes are reduced to about one-half of those in the neighbourhood of Moscow.

The amount of disturbance near Moscow, in relation to the distance of a place from the central line, is nearly as follows:—

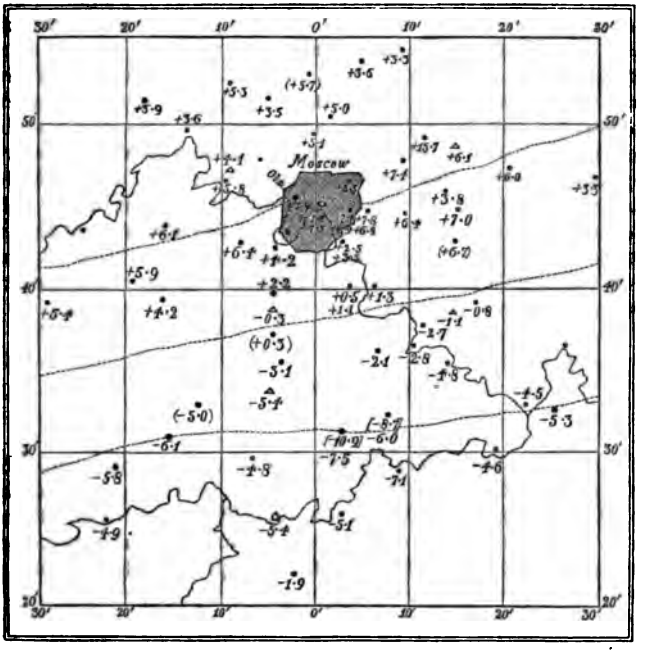
At distance	0	...	...	..	...	...	disturbance	=0
	3·8 versts	=2·5	English miles		...	...	...	2″·22
	12	...	=8	...	...	...	...	7·80
	20	...	=13	...	...	...	...	5·15
	26·59	...	=18	...	...	...	...	2·10
	34	...	=23	...	...	...	...	0

Its magnitude is sensibly the same, but with opposite signs, at equal distances north and south of the central line. The deviation at distances not exceeding 8 miles from the central line, is not less than $\frac{1}{60}$th part of the angle made by the verticals. Its sign is that corresponding to a deficiency of matter beneath the central line; and it does not apparently admit of explanation by any other supposition, as by an excess of density of the earth's crust in two places, one north and the other south, because beyond the limits above stated there is no trace of sensible disturbance.

According to Professor Schweizer's calculations, the volume of deficiency of matter, of the same density as the earth's crust, must be 1·2 cubic German geographical miles (the German geographical mile is $\frac{1}{15}$th of a mean degree of latitude). This supposes that the deficiency is at no great depth below the central line. If the density of the earth's crust, in that part, be only partially diminished, the volume of matter so affected must be increased in a corresponding proportion.

M. O. Struve, struck with the importance of these observations, and with the probable advantage of pursuing them

further, has applied for, and has obtained, from the Imperial Government, the means of enabling Professor Schweizer to examine the local disturbances at numerous points, in a district of considerable extent, connected with the general survey by a secondary triangulation. The small map below embraces but a portion of the country to be thus examined. It is intended that the comparison of observed and geodetic co-latitudes shall



The shaded space is the city of Moscow. The observatory is in its upper left-hand corner.

The figures at other stations denote the excess of observed co-latitudes, in seconds. Those inclosed in brackets are less certain.

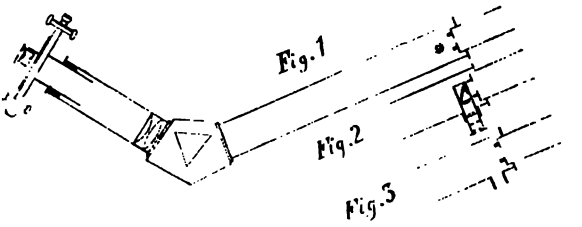
be extended, especially to the east and west, until the discrepancies disappear. When these limits are ascertained, it is intended, on Mr. Struve's suggestion, that longitudes and azimuths shall be observed at numerous stations. The elevation of different points, and the general form of the district, will also be ascertained by levelling, in order to eliminate the very small effects which may be produced by the inequalities of the surface. And pendulum-experiments are to be made, for ascertaining the effect of the disturbing cause on the force of gravity.

The extension of the survey is already begun by the Imperial General-Staff (Etat-Major). And for the other operations, Professor Schweizer will receive all the assistance which the influence of Russian scientific bodies can obtain.

The President remarked on this interesting communication, that it was one of great value, not merely for the importance of the results obtained, but also as an instance of extraordinary combination of different powers of different officers, directed each in the most advantageous way. Numerous instances had occurred of anomalies in the direction of gravity, but there had been few instances in which a superior officer had at once perceived, as Mr. Struve had done, the importance of the phenomena thus revealed, and the advantage of tracing them further, not only by extension over a larger area, but also by the introduction of other classes of observation; in which application for further powers had been made; in which the Government had with so much liberality responded to the call; and in which, by the zeal and ability of the officer immediately intrusted, the results, as far as they had been pursued, had been so happily worked out, and results of even greater importance were in prospect.

G. B. A.

The Astronomer Royal gave an oral account, illustrated by drawings, of the apparatus which had been prepared at the Royal Observatory, Greenwich, for the observation of the spectra of stars, and of some of the principal results obtained. In the drawing below, the first figure represents the entire ap-



paratus, in section; the second and third figures represent the right-hand portion of the apparatus in a section through the axis of the long tube, but at right angles to the former section.

In fig. 1, the smaller tube at the right-hand is for insertion in the socket upon the breech-piece of the telescope of the great equatoreal. To carry the apparatus steadily, (great accuracy of position is not needed) a temporary frame is screwed to the

sides of the telescope tube. The right-hand tube is thrust into the socket so far that the image of the star is formed in the moderately small hole in the inner end of that smaller tube. The pencil of light from the object-glass, which has converged to form the image of the star, then diverges and falls in a wide and divergent state upon the prism; after emergence it is received on a combination of lenses, which causes the pencils for the different colours to converge. It is seen thus that a spectrum may be formed at the place of the micrometer; it is necessary now to explain how breadth is given to the spectrum.

For this purpose the prism is not placed in the position of minimum deviation, or the position in which the angles of incidence and emergence are equal. It is so inclined that the angle of emergence is greater than the angle of incidence. With this arrangement, the change in the degree of divergence of the extreme rays of the pencil is different as regards (on the one hand) rays in the upper and lower parts of the pencil, or in the plane of the paper, and (on the other hand) rays in the plane perpendicular to the paper. The difference is such that, when after passing through the lenses the former have converged so as to form a pure spectrum at the place of the micrometer, the latter have passed convergence, and therefore the spectrum has breadth. The position of the prism, which gave a convenient breadth to the spectrum, was determined by trial. It is necessary to remark that the lenses must not be achromatic; they are in fact made of dispersive flint glass; if achromatic, the spectrum would have been formed in a plane inclined to the plane of the micrometer. The whole of these peculiarities were calculated numerically on geometrical theory before the construction was attempted.

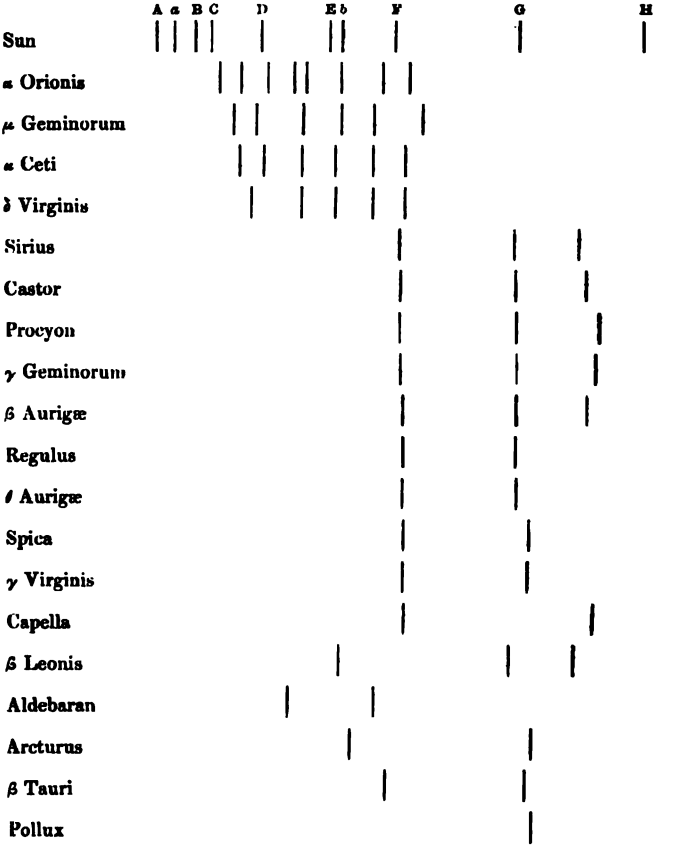
This principle may be regarded at present as experimental. In simplicity of construction it is much superior to the principle of causing the pencils which pass through the prism to consist of parallel rays, which requires the use of two achromatic object-glasses and one cylindrical lens; but it is not yet certain whether or not it defines the spectral lines with equal sharpness.

It is necessary now to explain how reference is made to the lines of the solar spectrum. In fig. 1, near the right-hand extremity of the large tube, a small circle may be seen. This is a view (not properly in section) of a small socket, which in fig. 3, lower side, may be seen empty; and which in fig. 2 contains a small eye-piece. It is thus used. A tube, containing at its end a very small hole (possibly $\frac{1}{500}$ inch in diameter) is thrust, in a definite position, into the right-hand tube of fig. 1, till the small hole is in the focal plane of the telescope-image. In this state the telescope is turned to the sun; the small hole represents a star of solar matter; and its spectrum may be observed and measured. An eyepiece with diagonal reflector, and carrying a wire in its focus, is thrust into the small socket, as in fig. 2, and the position of the solar hole with

reference to the wire is carefully observed. Then, when a star is to be observed, the small eyepiece is thrust in, the telescope is moved till the star's image has the same position in regard to the wire which the solar hole had, and then it is certain that the star occupies precisely the place of the solar hole; then the small eyepiece is withdrawn, and the spectrum is observed and measures are made with the micrometer.

For reading the comb of the micrometer there is an annular reflector to illuminate the field, not shown in the drawing.

The following sketch exhibits lines whose positions have been measured by Mr. Carpenter; numerous lines have been seen but not measured.



On this sketch Mr. Carpenter remarks:—

This diagram is only intended to show the positions of the

lines; no attempt has been made to illustrate their characters or their different intensities. The stellar lines are generally nebulous, resembling the solar line H; in the first four stars on the diagram they are, however, much more diffused on the side opposite to the red end of the spectrum.

Since these observations were communicated to the Society two more lines have been detected in the spectrum of *Arcturus;* one coincident with solar D, the other between it and solar C.

G. B. A.

Lines in the Solar Spectrum, as observed in the Balloon Ascent, 31st March last. By J. Glaisher, Esq. F.R.S.

The Astronomer Royal lent me the same apparatus as was used by Professor Smyth on the Peak of Teneriffe, for the purpose of observing the black lines in the spectrum. It consists of a prism, a fine adjustable slit placed in the focus of an object-glass of 14 inches focal length, and a telescope of the same length directed to the prism. No angular measure was contemplated, only eye observations and comparison of differences in the spectrum seen on the earth and at different distances from it in the course of the journey.

On the earth before leaving, a careful examination of the sky spectrum showed B as the limit of the red end, and some distance beyond G as that of the violet end, and all the principal lines.

The balloon left the earth on March 31, at a quarter past four P.M. At the height of half-a-mile the spectrum showed a general correspondence with that seen before starting, but I thought that G was less distinct and B was certainly less so.

At the height of a mile the spectrum was bright, but was shorter both at the red and violet ends, G was quite the limit, B was not visible, and C was doubtful.

At the height of two miles G was entirely lost, I could see F and D, but not beyond.

At the height of three miles the spectrum was very short, I could see a little beyond D to E, F was quite lost.

At the height of four miles I could see a little yellow tinge, but no lines.

At the height of four and a half miles I had no spectrum at all, even with a very open slit.

All those spectra were from the sky at an elevation of 60°; as far as time permitted I looked at a horizontal spectrum, and I did not see any marked difference between it and the spectrum at the higher elevation, at the same time. The general results

of these experiments are, that no line was lost from the spectrum, except through the loss of the spectrum itself, and that the spectrum shortened with increase of elevation.

The shortening of the spectrum with increase of elevation may have been, and most likely was, owing to want of light, (although to my senses there was abundance), the sky was of a deep dark blue, the sun was low, and it is possible the light was insufficient. For this class of observations it will be necessary to have a morning or mid-day ascent, for comparison of results with the preceding as well as to determine whether really the spectrum shortens with elevation or not.

Results of the Meridional Observations of Small Planets; Occultations of Stars by the Moon; and Phenomena of Jupiter's Satellites; observed at the Royal Observatory, Greenwich, in the months of February and March, 1863.

(*Communicated by the Astronomer Royal.*)

Astræa (5).

Mean Solar Time of Observation.		R. A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Feb. 3	13 23 41·3	10 18 33·47	78 57 20·09
12	12 41 48·1	10 12 2·42	77 44 56·16
13	12 37 5·1	10 11 15·20	77 36 35·35
16	12 22 53·7	10 8 51·07	77 11 32·87
21	11 59 11·7	10 4 48·03	76 30 14·05
27	11 30 55·5	10 0 6·47	75 43 7·56
28	11 26 15·0	9 59 21·72	75 35 41·74
Mar. 3	11 12 19·1	9 57 13·25	75 14 10·08
4	11 7 42·5	9 56 32·37	75 7 17·41
11	10 36 1·6	9 52 22·20	74 24 7·20
16	10 14 8·0	9 50 7·74	73 59 2·40
17	10 9 50·2	9 49 45·77	73 54 41·46
21	9 52 57·5	9 48 36·48	73 39 18·79
23	9 44 42·1	9 48 12·83	73 32 56·47
24	9 40 37·0	9 48 3·63	73 30 6·51
25	9 36 34·1	9 47 56·65	73 27 27·25
26	9 32 32·8	9 47 51·26	73 25 0·76
27	9 28 33·7	9 47 48·07	73 22 49·03
31	9 12 55·6	9 47 53·61	73 16 6·62

Victoria (12).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Feb. 13	11 37 22·5	9 11 22·76	87 12 36·29
16	11 22 41·0	9 8 28·53	86 57 52·78
17	11 17 48·3	9 7 31·56	86 52 43·48
28	10 24 59·3	8 57 55·96	85 53 2·85
Mar. 3	10 10 57·2	8 55 41·27	85 36 17·41
17	9 8 8·4	8 47 53·81	84 20 40·25
21	8 51 6·2	8 46 35·02	84 1 4·52

Irene (14).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Feb. 12	13 29 29·6	10 59 51·76	67 36 19·84
13	13 24 52·2	10 59 10·11	67 27 13·42
14	13 20 13·6	10 58 27·32	67 18 10·24
16	13 10 52·7	10 56 57·95	67 0 18·92
17	13 6 10·7	10 56 11·76	66 51 32·73
28	12 13 38·3	10 46 52·86	65 26 2·62
Mar. 3	11 59 12·6	10 44 14·40	65 7 17·03
4	11 54 24·2	10 43 21·80	65 1 32·69
17	10 52 52·5	10 32 55·12	64 13 35·83
21	10 34 33·2	10 30 19·08	64 9 20·45
23	10 25 32·8	10 29 10·25	64 9 1·52
24	10 21 4·8	10 28 38·11	64 9 20·63
25	10 16 38·9	10 28 8·04	64 9 57·18
26	10 12 14·5	10 27 39·47	64 10 51·05
27	10 7 51·8	10 27 12·60	64 12 2·57
31	9 50 39·6	10 25 43·74	64 19 39·93

Urania (30).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Feb. 5	8 9 48·4	5 11 42·11	65 36 22·02

Ariadne (43).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Feb. 14	10 48 27·4	8 26 16·21	75 7 34·27
17	10 33 49·5	8 23 25·59	74 58 8·07
18	9 42 4·7	8 14 54·32	74 27 39·58

Bellona (36).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Feb. 3	12 29 22·6	9 24 5·86	76 53 46·05
12	11 46 46·2	9 16 51·42	75 34 43·19
13	11 42 2·7	9 16 3·78	75 26 1·69
14	11 37 20·0	9 15 16·81	75 17 20·50
16	11 27 55·9	9 13 44·24	75 0 13·22
17	11 23 15·2	9 12 59·34	74 51 45·05
21	11 4 39·4	9 10 6·77	74 18 46·00
27	10 37 17·3	9 6 19·39	73 32 34·51
28	10 32 47·5	9 5 45·45	73 25 22·98
Mar. 2	10 23 53·4	9 4 42·97	73 11 20·49
3	10 19 28·9	9 4 14·34	73 4 33·56
4	10 15 4·6	9 3 45·84	72 57 57·15
17	9 20 23·8	9 0 11·22	71 46 54·03
21	9 4 32·5	9 0 3·55	71 30 59·42
23	8 56 47·2	9 0 10·14	71 24 7·74
24	8 52 57·3	9 0 16·08	71 20 55·13
25	8 49 9·2	9 0 23·92	71 17 55·65
26	8 45 22·3	9 0 33·00	71 15 0·49
27	8 41 37·8	9 0 44·41	71 12 25·96
31	8 26 55·2	9 1 45·65	71 3 29·85

Europa (52).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Feb. 3	10 4 41·3	6 59 0·82	70 13 4·54
9	9 38 14·0	6 56 8·47	69 50 33·65
11	9 29 36·9	6 55 23·03	69 43 28·00
13	9 21 5·6	6 54 43·51	69 36 30·49
14	9 16 52·4	6 54 26·17	69 33 5·47
16	9 8 30·7	6 53 56·13	69 26 27·75

Diana (78).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Mar. 27	11 29 23·7	11 48 57·86	96 56 25·75
31	11 10 0·4	11 45 17·59	96 46 33·45

All the observations of N.P.D. have been corrected for Refraction and Parallax.

Occultations of Stars by the Moon.

Day of Observation. 1863.	Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
Feb. 11	15 Geminorum, disapp.	Dark	9 43 15·8	S.
Mar. 2	α Cancri, disapp.	Dark	11 48 54·8	C.

The star, the occultation of which was observed on Jan. 27, and printed without a name in the last number of the *Monthly Notices*, page 174, was *δ Arietis.*

Phenomena of Jupiter's Satellites.

Day of Observation. 1863.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
Feb. 16	I	Eclipse, disapp.	14 2 27·1	E.
17	I	Ingress, first cont.	12 3 35·7	R.
,,	I	,, bisection	12 6 35·2	R.
,,	I	,, total ingress	12 10 34·6	R.
,,	I	Egress, first cont.	14 14 42·9	R.
,,	I	,, bisection	14 18 42·3	R.
,,	I	,, last cont.	14 21 26·8	R.
Feb. 28	II	Eclipse, disapp.	12 42 6·2	E.
Mar. 11	I	Eclipse, disapp.	14 10 32·1	D.
25	II	(a) Eclipse, disapp.	9 43 25·3	C.
,,	II	Occ. reapp. bisection	12 59 40·7	C.
,,	II	,, last cont.	13 0 55·5	C.
26	III	Eclipse, disapp.	13 20 57·1	J. C.
27	I	Eclipse, disapp.	12 25 34·9	D.

(a) The satellite extremely faint, the observation very unsatisfactory.

The initials S., D., E., C., J. C., and R., are respectively those of Mr. Stone, Mr. Dunkin, Mr. Ellis, Mr. Criswick, Mr. Carpenter, and Mr. Roberts.

Note on the Appearance of the Dark Ring of Saturn.
By J. Carpenter, Esq.

During the observation of *Saturn* with the transit-circle, on the evening of March 26th, 1863, it struck me that the dark space between the ring and the ball seemed much contracted. Upon looking at it with the Equatoreal I found that this arose from a great increase in the brightness of the dusky ring, which appeared nearly as bright as the illuminated ring, and might easily have been mistaken for a part of it.

Occultation of B.A.C. 4006 *by the Moon, observed at Forest Lodge, Maresfield, by Capt. W. Noble, on Thursday, April* 2, 1863.

The star disappeared at

$$12^h\ 9^m\ 46^s{\cdot}6 \text{ L.S.T.} = 11^h\ 26^m\ 33^s{\cdot}62 \text{ L.M.T.}$$

Owing to the obliquity of the Moon's motion over it, and the immersion taking place near a high mountain, the star presented the appearance of a little sun setting; its spurious disc apparently descending and touching the lunar limb, just before its instantaneous disappearance. The reappearance was not observed.

Telescope employed: my Ross Equatoreal of 4·2 inches aperture, with a power of 255 adjusted on the star.

Note on a Small Companion of Procyon, &c.
By J. Gurney Barclay, Esq.

Towards the close of December, 1855, my attention was directed to the neighbourhood of *Procyon*, by a letter to the Editor of the *Times* from Mr. Hind, dated 21st of that month, in which he stated, that he strongly suspected the existence of some star or other body in the immediate neighbourhood of *Procyon*, by which the proper motion of that star was irregularly affected.

Being at that time in possession of an excellent Refractor, by Cooke, of 7½ inches aperture, I took the earliest opportunity of closely inspecting the neighbourhood of *Procyon*, and on the 10th January, 1856, I discovered a small star within the blaze of the light of the larger one, and which I roughly estimated at from 3 to 4 seconds of time by the sound of the clock preceding *Procyon* in R.A., and but little removed to the North in December.

I communicated these particulars to Mr. Hind, accompanying them with a rough sketch of the relative position of the small star with the large one, and two others of small magnitude in the immediate vicinity. Mr. Hind having recently again called my attention to this object, I requested Mr. Romberg, who has the care of my Observatory, and which now contains a 10-inch Refractor by the same maker, to give it his best attention, and to endeavour to determine with some accuracy the exact position of the small star. This he has done, and given the result in the accompanying paper. I may add, that in order to get a very distinct view of the object, we found it necessary to make use of a ring micrometer, by which

the rays of light of the large star were shut off, by being brought behind the dark ring.

The other two minute stars which he notices, are clearly fainter than that above described, but are distinctly visible with the 10-inch aperture in the absence of the large star from the field.

Mr. Romberg has also given his attention to the measurement of the triple star of γ *Andromedæ*, and has succeeded in obtaining some satisfactory results.

Leyton, Essex,
24th March, 1863.

Measures of the Small Star near Procyon.
By Hermann Romberg.

I determined the position of this small star, which was first seen by Mr. Gurney Barclay in the year 1856, in reference to *Procyon*.

			° ′		″	
	1863, March	17	295 18	(6)		
		18	295 40	(6)		
		19	294 25·5	(6)	44·9	(12)
		21	294 14·1	(6)		
		23	294 47·5	(6)	46·9	(9)
Mean	1863·22		294 54	(30)	45·8	(21)

I estimate the small star to be of the 10.5 magnitude.

Measures of the Companion of γ Andromedæ.
By Hermann Romberg.

I attacked this star with Mr. G. Barclay's excellent Refractor, during the winter, on several occasions, but was never very successful; but since the 17th of February, shortly after sunset, the star was shown clearly divided every time I looked at it with powers 750, 500, and 330. By means of a roughly introduced spider's line I obtained the following measures of position: —

			° ′	
	1863, Feb.	27	107 41	(12)
		Mar. 18	105 43	(6)
		19	107 10	(4)
Mean	1863·20		107 3	(22)

I estimated the distance to be about 0″·6, and the magnitudes of the stars 5 and 6.5.

Mr. Gurney Barclay's Observatory,
Leyton, 1863, *April* 7.

Observations and Elements of Comet I. 1863.
By Hermann Romberg.

I have obtained the following observations of the Comet I., 1863 :—

	M.G.T.	Δα Comet — Star.	Δδ Comet — Star.	
	h m s	m s	′ ″	
1863, Jan. 11	15 55 59·5	+9 15·230	+5 13·5	*a*
20	17 51 59·4	0 41·951	+7 9·4	*b*
24	17 58 35·5	1 36·834	+4 54·3	*c*
Feb. 3	16 59 16·0	0 17·832	−4 30·0	*d*
	17 39 9·5	+0 22·540	−5 18·1	*d*

The apparent places of the stars of comparison for the date of observation are,

	h m s	° ′ ″	
(*a*)	17 41 19·20	+38 56 8·5	Radcl.
(*b*)	19 16 28	34 2 0	Lal.
(*c*)	19 38 21·41	31 45 11·7	B. Z.
(*d*)	20 17 5·88	+26 41 30·8	B. Z.

The star (*b*) is given in Argelander's Northern Zones, Zon. +34°.

On Feb. 3d I compared the Comet with another star, and obtained, for the above times,

		m s	′ ″
Feb. 3		−1 34·568	−3 20·2
		−1 29·660	4 11·1
	18h 22m 5c·0	−1 24·547	−5 6·1

The apparent place of this latter star is nearly

20h 18m 28s·19 +26° 40′ 22″·3

The differences in Right Ascension and Declination are corrected for Refraction and Parallax ; they are the means of at least six transits. The Comet's appearance was round and faint ; but I discerned from the 20th of February a faint nucleus, which had on the 3d of February increased consider-

ably in brightness. Unfavourable weather in the morning hours prevented me from observing it any further.

I calculated the following parabolic elements from the observations, Berl., Dec. 5, Dec. 26, Leyton, Jan. 11, taking into the account the corrections of Aberration and Parallax:—

T	1863, Feb. 3·48790 M.G.T.	
	° ′ ″	
π	191 22 12·4	Mean Eq. 1863·0
Ω	116 55 19·7	
i	85 21 34·2	
log q	9·9002614	
	Direct.	

These elements give for the mean place the error

C − O $\Delta\alpha = -0''\cdot7$ $\Delta\delta = +7''\cdot1$

On a mode of Figuring Glass Specula for the Newtonian Telescope. By the Rev. Henry Cooper Key.

As the subject of silvered glass specula is now beginning to attract some attention, I have taken the liberty of laying before the Society a brief account of the successful manufacture of these specula, including a method which I have suggested for producing with great ease and certainty the true parabolic figure necessary for first-rate performance.

The machine used for the purpose is the same in principle as that of the Earl of Rosse, and very similar in detail, but with some slight modification in the speed of the various parts. The number of strokes to one revolution of the speculum-plate is 104. The pin of the eccentric giving the side motion is carried from side to side with an unequal velocity by means of an oval wheel, whereby the centre is passed over rapidly and the action is prolonged at the two extremes. The number of revolutions of the side-motion eccentric to one revolution of the speculum-wheel is $5\frac{1}{2}$. The amounts of stroke and side-motion found to produce the best results are stroke = $\frac{1}{3}$ diameter of the speculum and side-motion $\frac{1}{4}$ the same; but precision on this point does not appear to be very material.

The glass disc is ground upon a cast-iron tool, of the same diameter as itself, carefully turned to the guage, and scored at right-angles into facets $\frac{5}{8}$-in. square; these grooves are filled up with wax or pitch until the rough grinding is complete, or nearly so. It is not absolutely necessary to work a concave iron tool upon the convex, the simplest plan being to figure

the convex tool by means of the speculum itself; or, if more than one is to be worked, by grinding them alternately upon it. The convex tool should be turned to a curve giving about 6 inches less focal length than that ultimately desired, to allow for the lengthening that will take place in the course of the grinding.

The fine grinding is effected by means of emery of six different degrees of fineness, commencing with flour-emery as No. 1; these are, of course, obtained by washing and pouring off the finer particles while in suspension. The sixth, or finest quality, takes between two and three days to settle. The last application is slate-mud, carefully washed, and mixed with a very little rouge, also washed. Rouge always contains some amount of grit, and should never be used unwashed.

As the character of the spherical figure, upon which the quality of the future mirror entirely depends, is given in fine grinding upon the iron tool, it is advisable to prolong the process of fine grinding considerably, and to bring the surface, in fact, to such a fine degree of semi-polish, that, when placed with wet rouge on the pitch-tool, it shall come up to a perfect polish in from three to four hours. The shorter the sojourn of the speculum on the pitch-tool the better. I need scarcely add that no rings of unequal polish ought to appear during any part of the fine grinding.

The pitch polishing tool may be made either of two pieces of seasoned wood glued together across the grain, or of slate. It should be turned to the guage, and, as it should be of the same size as the speculum when coated with pitch, it must be about $1\frac{1}{2}$ inches larger in diameter, to allow of the margin being perfect. The pitch is hardened with resin, until the thumb-nail can make little or no impression upon it when cold: the harder it is the better, provided it be not too brittle. It is to be scored, while still warm, into squares or facets, which, after some hours' work upon the machine, shall not exceed $\frac{1}{2}$-in. square on the surface, and shall stand at least $\frac{1}{8}$-in. high, the base of each facet being not less than $\frac{7}{8}$-in. square. It is of great importance that they stand well detached from one another, that they may give equally to pressure in all directions. In polishing, as well as in grinding, the speculum is placed uppermost.

The speculum, when polished and placed in the tube, and directed to a fine watch-dial at about 100 yards' distance, will now show that the extreme marginal rays come to a focus about $\frac{1}{4}$-inch inside the central rays—proving thereby a spherical figure.

To give the parabolic figure, which shall unite all the rays in one precise focus, proceed with a sharp chisel to cut the facets of the pitch-tool smaller, leaving the four centre ones untouched, and gradually diminishing the surface of the others over the whole face of the tool, until those at the margin are

reduced to $\frac{1}{4}$-inch square, or even a little less. The proper amount of graduation is obtained by experience, and upon the judicious execution of this the quality of the speculum will depend. The speculum is now placed upon the graduated tool, and the action of the machine continued with the same amount of stroke and side-motion as before, until, supposing its surface divided by diaphragms into four nearly equal zones—outer, second, third, and central—it gives equally perfect definition on the dial, and bears an equally high power, with each zone, at precisely the same focal point. This is attained usually in a few hours, without any difficulty. I ought perhaps to add, by way of caution, that the stroke and side motion must not be altered in amount in the slightest degree from the commencement of the fine grinding to the completion of the figure, and more especially the side motion.

This is the normal mode of procedure; but, by way of precaution, it is advisable to graduate the pitch-tool very slightly before commencing the polishing; and when that is quite perfect (and not the slightest granulation must be left visible), the tool is to be fully graduated, as before stated.

My first experiment with the graduated tool, when the idea occurred to me, was made on a half-polished mirror, and in two and a half hours the definition became such as to surprise me. With a half-polished mirror, unsilvered, 12-inches aperture and power of 1000, the dial was exquisitely defined; with 350 the effect was that of viewing the dial itself close to the eye without the intervention of a telescope; and a fly resting on the well-focussed dial remained itself out of focus, with the exception of its feet, forcibly reminding me of the definite focal plane of a good achromatic microscope: the mirror, in fact, came up to focus and went out—in an instant—with the sharp precision of a microscope.

No more severe test is required, or indeed can be found, to insure fine performance on celestial objects, than the dial, and the mode of procedure above mentioned.

The speculum is to be silvered by Liebig's process; and the coat should be rather thin, as, if thick, it is liable to lose its fine figure by rubbing up. The silver will never tarnish if kept dry. A small bag should be kept in the same box with it, containing sawdust which has been saturated with chloride of calcium, and dried.

Circumstances over which I had no control have prevented my own further progress, and, indeed, I shall be delayed some weeks longer before I am able to resume the work; but having, last autumn, communicated my method to Dr. Frankland, of the Royal Institution, and to Mr. With, Master of the Bluecoat School at Hereford, I am able to state what they have accomplished.

Dr. Frankland immediately and readily figured a speculum of 7 inches' aperture, whose spherical figure was not good (it

having been polished by hand on paper), so that it would split 36 *Andromedæ* with 210, clearly and sharply. He, however, expects superior results from a machine-made speculum, and speaks in the highest terms of my suggestion of the graduated pitch-tool.

Mr. With, of Hereford, in his small intervals of leisure during the last three months, has completed three or four glass specula of the highest class, $6\frac{1}{4}$-inches aperture and 6-feet focus, and $5\frac{1}{2}$-inches aperture and $4\frac{1}{2}$-feet focus. With the larger telescope he is able to elongate the disk of the small companion of γ *Andromedæ* readily. Its definition of a large fixed star is remarkably fine, giving a perfectly round clean disk, surrounded at a little distance by one faint concentric ring, and without any other appendage or false light whatever.* This was his first attempt. One of his $5\frac{1}{2}$-inches specula is, however, slightly superior to this, and is presumed to be perfect: not the least trace of error of figure can be detected over the entire surface after the strictest scrutiny.

The belief is, after sundry trials and comparisons with achromatics, that the silvered-specula Newtonian is very little, if at all, inferior in efficiency to the finest achromatic of the same aperture. Should this be confirmed by future experience, as I fully anticipate it will be, the former will prove to be formidable rivals to the latter, as the cost of making is not likely to amount to one-eighth or even one-tenth of the achromatic object-glass.

I have omitted to mention that excellent small glass flats for silvering may be obtained from the common French plate-glass—the best from the middle of the plate.

Should any member of the Society desire further information beyond what I have been able to convey in these brief notes, I shall be happy to give it at any time.

Stretton Rectory, Hereford,
March 9th, 1863.

Companion of Sirius.

(*Extract of a Letter from the Rev. W. Dawes.*)

"Goldschmidt's star *d* near *Sirius* is easy enough, but have not been able to perceive any of the others. In fact, suc

* The fifth star in the trapezium in *Orion* is visible as a steady obje and the sixth in moments of fine vision. The distant comes of λ *Orionis*, mentioned by Smyth, is an easier object in this telescope than the fifth s in the trapezium. In the triple star ζ *Orionis* the separation between t close pair is equal to one diameter of the large star with a power of 220.

an object is far less a *telescopic*, than an *atmospheric test* in these latitudes. The star should be examined at the Cape of Good Hope."

Hopefield, Haddenham, 10th April, 1863.

Mr. Carrington, in reference to the subject of the Motion of the Solar System in Space, remarks as follows:—

"I have just become acquainted with the circumstance, that in 1859 a Royal Medal was offered for competition in the University of Christiania for the best Memoir to be received in 1860, containing an investigation whether the mean direction of motion of the Comets before coming under solar influence differed from the concluded motion of the Sun, and that the award was made to Candidate Mohn for a Memoir just published under the professorial superintendence of Prof. Fearnley. Very curiously, as it appears to me, the attack is conducted by the only way which I can conceive different to the process I followed myself in 1860, in a short Memoir which the Society did me the honour to publish. Mr. Mohn, after selecting 173 orbits, and throwing out the periodical ones, searches to find whether there is any region of the heavens approaching a great circle belt, in which the poles of the several planes of motion are chiefly congregated, and finding such a belt of preponderance and fairly regular distribution, points out that the axis of this belt will be the general direction of cometic motion as defined. His conclusion is, that the direct and retrograde Comets concur in giving a mean direction of motion towards long. 0° lat. + 10½°, or nearly at right angles to the general conclusion for solar motion.

"But Prof. Fearnley, who appears, as proposer of the Problem in Christiania, to take much personal interest in it, points out in his commentary that the direction found in the Memoir is greatly attributable to the special circumstances attending the discovery of Comets, which, it will be remembered, I had also occasion to dwell upon in considering the conclusions of my own investigation. These are

"1 The discovery generally between sunset and midnight.

"2 Observers mainly in northern hemisphere.

"3 The heliocentric elongation of the Comet being commonly acute.

"He goes so far as to say, that examining the effects of these circumstances, he obtains, as a direct and necessary consequence therefrom, the result arrived at by Mr. Mohn.

"The attack on this problem by way of the orbits appears, therefore, by two independent inquiries, to be hopelessly

nugatory; and it becomes the more desirable that the older route of research should be promoted by the improving the data needful for its success, namely, well determined stellar residual motions; the whole subject at present seeming, on critical examination, to be less advanced than once supposed, though presenting hopes of successful future research."

Minor Planet (77).

The discovery of this minor planet by Dr. Peters, and his observations of it Nov. 1862, are mentioned *Monthly Notices*, *ante*, p. 76, and in the *Ast. Nach.* No. 1399. No further observations appear to have been made, nor any elements computed.

Minor Planet (78), *Diana.*

The discovery, by Dr. Luther, Director of the Observatory of Bilk, is announced *Ast. Nach.* No. 1408; the planet was seen of the magnitude 10, the position being

1863, March 15, $13^h\ 24^m\ 14^s{\cdot}4$ Bilk M.T. R.A. $180^\circ\ 12'\ 6''{\cdot}8$.
Decl. $-\ 7^\circ\ 20'\ 3''{\cdot}4$.

the retrograde motion being more than 2 seconds of time in the hour.

The following elements, calculated from the Bilk Observation of 15 March, and from observations at Bonn, March 23 and 31, have been calculated by Herr Tiele of Bonn, *Ast. Nach.* No. 1410.

Epoch, 1863, March 15·0, Berlin M.T.

$$\begin{aligned} M &= 39^\circ\ 50'\ 29''{\cdot}0 \\ \pi &= 121\ \ 6\ \ 30{\cdot}3 \\ \Omega &= 334\ \ 1\ \ 19{\cdot}3 \\ i &= 8\ \ 39\ \ 4{\cdot}8 \\ \varphi &= 11\ \ 59\ \ 38{\cdot}8 \\ \mu &= 833''{\cdot}877 \\ \text{Log } a &= 0{\cdot}419270 \end{aligned}$$

Equinox, 1863·0.

RECENT PUBLICATIONS.

Untersuchungen über veränderliche Eigenbewegungen von G. F. J. Arthur Auwers. Erster Theil: Bestimmung der Elemente der Procyonbahn und Untersuchung der für veränderlich gehaltenen Eigenbewegungen von α Virginis, β Orionis und α Hydræ. Nebst einem die Reduction eines Theils der Beobachtungen an den Mittagsfernröhren von Greenwich und Palermo von 1765 bis 1810 enthaltenden Anhang. Inaugural Dissertation. 4°, Königsberg, 1862, pp. 1–96.

The researches on Procyon, *Ast. Nach.* Nos. 1371 73 (an abstract of which is published, *Monthly Notices, ante*, pp. 18–20), are, in fact, an extract from this elaborate memoir; the title shows what are the other stars to which M. Auwers has extended his investigations.

For Sale: A Marine Box Chronometer, the property of Mr. Carrington. Price 18*l.* It was cleaned and put in thorough order last year, and may be seen, and its rates inspected, at Mr. Frodsham's, in the Strand, where it has since remained, from the owner having no further use for it.

ERRATA

In No. 5 of the Monthly Notices, Vol. XXIII.

Page 165 (heading), *for* Latitude, *read* Longitude.

— —, lines 1 and 2, *for* Kingston, *read* Kingstown.

— —, line 28, *for* R. J. Leckie, *read* R. J. Lecky, Esq.

— 166, — 22, *for* Knightstown, *read* Kingstown.

— 167, — 20, *for* apparent movements of the bodies in space, *read* apparent movements on the surface of a globe to the real movements of the bodies in space.

— 168, — 11, *for* 524°·54, *read* 514°·54.

— —, — 17, *for* 15° 31′, *read* 15°·31.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIII. *May* 8, 1863. No. 7.

G. B. AIRY, Esq., Astronomer Royal, in the Chair.

Henry Johnson, Esq., 39 Crutched Friars;
Celestin Baume, Esq., Baume Lodge, East Brixton; and
Lieut. Geo. Lyon Tupman, Royal Marine Artillery,
were balloted for and duly elected Fellows of the Society.

On Solar Spots, and on the Variable Star η (Argo) Navis. By Prof. Wolf.

(*Translation of part of a Letter to the Astronomer Royal, dated Zurich, 1863, April 2.*)

"In a short time I shall have the honour to lay before you the fifteenth number of my communications concerning the Solar Spots. At present I give you its principal contents, and I shall be much rejoiced if you consider this sufficiently interesting to be communicated to the Royal Astronomical Society. In the first place, I have determined, by means of my own observations, together with those of Herr Schwabe at Dessau, and Herr Jenzer at Berne, the mean relative number for the year 1862, and I have now for

	1858	1859	1860	1861	1862
the relative numbers	50·9	96·4	98·6	77·4	59·4

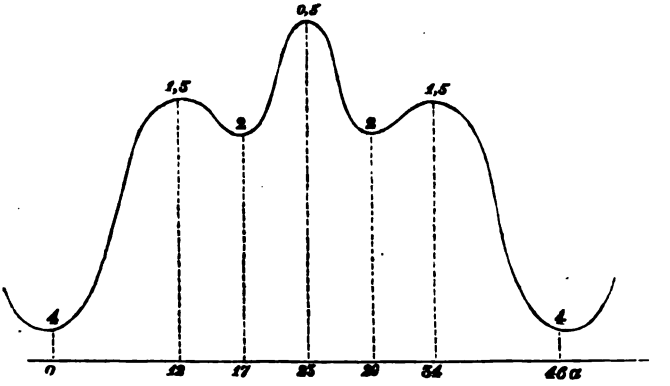
so that the Maximum which I had determined for 1860·2 is already very beautifully shown in these numbers. From the number, 59·4, corresponding to 1862, I have obtained, according to the formulæ laid down by me, for the year 1862, for Munich 9′·27, and for Prague 8′·38, as the mean declination variation. In the second place, I have not to enter here into some additions to the collection of formulæ in No. 13, in conjunction with one of my colleagues, Herr Fritz, made a new comparison between the phenomena of the Solar Spots and of the Northern Lights, which has yielded very interesting results. That is to say, evidence is afforded by it that the two phenomena form parallel curves, and not only does the *eleven-year period* show itself in the Aurora, but also the *greater period of about* 56 *years*, which I have referred to in No. 12 as becoming apparent in the case of the Solar Spots. By this labour, therefore, the secular period of the Aurora Borealis is at the same time established—a point on which no satisfactory conclusion has hitherto been arrived at.

"Besides these labours on the phenomena of the Solar Spots, I have recently obtained, when occupying myself with the Variable Stars, and having collected amongst others the observations of η (*Argo*) *Navis*, a result which may be interesting to the Astronomical Society, and especially to Sir John Herschel. I found, contrary to the hitherto generally supposed irregularity, that a period of about 46 years could be attributed to it, as soon as two consecutive Maxima and Minima had been inserted, as the annexed light-curve indicates. There is, as it appears to me, nothing opposed to this in regard to β *Lyræ* and R *Sagittæ*, and the observations with which I am acquainted are quite regularly represented thereby, as is shown by the following comparison:—

	Hypothesis.		Observation.		
Phase.	Magnitude.	Time.	Time.	Magnitude.	Observer.
Minim.	4	1677	1677	4	Halley.
Minim.	4	1723			
Cons. Max.	1·5	1735			
Cons. Min.	2	1740			
Maxim.	0·5	1746	1751	2	Lacaille.
Cons. Min.	2	1752			
Cons. Max.	1·5	1757			
Minim.	4	1769			
:					
Minim.	4	1815	1815	4	Burchell.
			1824	2	Brisbane.
Cons. Max.	1·5	1827	1827	1·5	Burchell.
Cons. Min.	2	1832	1832	2	Johnson.
			1835	1·5	Herschel.

	Hypothesis.		Observation.		
Phase.	Magnitude.	Time.	Time.	Magnitude.	Observer.
Maxim.	0·5	1838	1838	0·5	Herschel.
			1842	1	Maclear.
Cons. Min.	2	1844			
			1845	1	Jacob.
Cons. Max.	1·5	1849			
			1850	1.	Gilliss.
			1856	1·5	Powell.
			1859	3·1	Ditto.
Minim.	4	1861	1861	3·6	Ditto.

"Possibly there may exist other observations unknown to me, which may overthrow my system, but the trial of my hypothesis would be always worthy of attention."



(*Note.* The figures 4, 2, &c. in the diagram evidently denote that the star's brightness will at these times be of the 4th magnitude, 2d magnitude, &c.—G.B.A.)

The Notes of Phase, which have been printed "cons." (consecutive) are in the original "sec.," conceived to have the same meaning.

The descriptions of Phase evidently ought to be these,—

Minimum.	Minimum.
Minimum.	1st Max.
1st Max.	1st Min.
1st Min.	Maximum.
Maximum.	2d Min.
2d Min.	3d Max.
3d Max.	Minimum.
Minimum.	

Note on the Coefficient of the Parallactic Equation in the Lunar Theory. By E. J. Stone, Esq.

The literal expressions of the coefficient of Parallactic Equation, developed to the sixth power of the small quantities, are identical in the theories of Plana, Pontécoulant, and Lubbock: the numerical value deduced with the usually received values of the constants, amounts to $122''\cdot1$; if we increase the mean Solar Parallax by a thirtieth part, the coefficient becomes $126''\cdot2$; the value deduced by the Astronomer Royal from the Greenwich Lunar Reduction is $124''\cdot7$; the value, however, given by the Altazimuth Observation alone amounted to $125''\cdot5$. The Astronomer Royal remarks in his Memoir that the Altazimuth Observations possess the rare advantage of having been extended to portions of every lunation very near to the conjunction of Sun and Moon, and thereby of discriminating between the entangled effects of Parallactic Equation and Variation. It would appear, therefore, that the lunar theory, as developed by these gentlemen to the sixth power of m, would be brought nearer to observation by increasing the mean Solar Parallax by a thirtieth part, but that the improvement (unless we adopt the value of the coefficient of Parallactic Equation deduced from the Altazimuth Observations) is by no means of a marked character. The coefficient of Parallactic Equation, given by Professor Hansen, is $121''\cdot4$, but the method of development adopted by him does not allow of direct comparison between his value and that deduced by Mr. Airy. It would be a point of interest to determine whether Professor Hansen's theory would bear any considerable increase in the mean Solar Parallax.

1863, *May 7th.*

On the Acceleration of the Mean Motion of the Moon.

In the *Berichte der K. Sächs. Gesellschaft zu Leipzig*, 15 April, 1863, is contained a short paper by Prof. Hansen, "Einige Bemerkungen über die Säcular-änderung der mittleren Länge des Mondes." The value of the acceleration, as calculated by Adams and Delaunay, is considerably—nearly $6''$—less than that employed in the author's Lunar Tables; such smaller value cannot, however (as he has shown, *Comptes Rendus*, t. l., 1860), represent the ancient eclipses; and assuming that it is the correct value of the acceleration arising from the change in the excentricity of the solar orbit, the deficiency must be accounted for by other causes. That which most naturally presents itself is a change in the length of the

sidereal day, or period of the Earth's rotation; and he remarks that Laplace's demonstration of the invariability of the sidereal day depends on the very assumption that the acceleration is *wholly* accounted for by the change of the excentricity of the solar orbit, and consequently must in the present question be put aside. He proceeds to show that the deficiency of 6″ in the acceleration would be accounted for by an alteration of the sidereal day by the almost infinitesimal increase of $0^s{\cdot}01197$ in the course of 2000 years; and he remarks that the same alteration would produce a secular alteration of about $1''{\cdot}9$, consequently a sensible effect in the mean longitude of *Mercury*, the motion of which planet, as shown by Leverrier, is not completely accounted for by existing theories. Would the supposition in question produce a better agreement of the theory of *Mercury* with observation? This question he considers so important, that he cannot help expressing the wish that it may be soon undertaken. But in the meantime it is remarkable that the value of acceleration, as obtained by the author, without the assumption of any such supplementary cause, should agree so well with the observations, and that the small changes in the values of the secular variations to which his latest calculations have led him should produce a still better agreement. He in fact finds that to the coefficients of the secular variations of his Lunar Tables there should be added

for Mean Longitude	$+ 0''{\cdot}365$
,, Perigee	$- 1{\cdot}352$
,, Node	$- 0{\cdot}464$

and he concludes by stating the effect of these changes on the eclipses of Thales, Agathocles, Stiklastadt, and Ennius, and on two eclipses in the years 1567 and 1605 respectively.

Note on Plana's Lunar Theory. By A. Cayley, Esq.

I have been much surprised to find that there is an error of the order $m^2\gamma^4$, arising from the omission of a factor $(1+\gamma^2)^{-1}$, in the expression for $\frac{d^2\delta u}{dv^2} + \delta u$, as given by the equation (II)′ (*Théorie de la Lune*, t. i. p. 267), being the equations made use of in the theory for the determination of δu, the perturbation of the reciprocal of the radius vector. This error may probably be the cause of some of the discrepancies in the terms of the fourth and higher orders, between Plana's results and those of Pontécoulant and Delaunay.

Plana's equation (6) t. i., p. 260, is

$$\frac{d^2 \delta u}{d v^2} + \delta u = \mathrm{a}\,\mathrm{R}''$$

$$+ f(e,\gamma)\,\mathrm{Q}'\,e \cos\left(c v - \int \varpi\, d v\right)$$

$$- \left\{ f(e,\gamma)\,(1+\gamma^2)\,(1+s_{,}^2)^{-\frac{3}{2}} - \frac{\mathrm{a}\,(1+s^2)^{-\frac{3}{2}}}{\mathrm{a}_{,}\,\psi(e,\gamma)} \right\}$$

$$+ f(e,\gamma)\,\mathrm{P}\,\gamma^2\,(1+s_{,}^2)^{-\frac{3}{2}}\,\Theta,$$

if for shortness

$$\Theta = \frac{3}{8}\gamma^2 - \left(1 + \frac{1}{2}\gamma^2\right)\cos\left(2 g v - 2\int \theta\, d v\right) + \frac{1}{8}\gamma^2 \cos\left(4 g v - 4\int \theta\, d v\right).$$

R″ (p. 256) should be

$$\mathrm{R}'' = \frac{1}{\sigma\,\mathrm{a}_{,}\,\psi(e,\gamma)}\,\frac{\Omega_2 - \frac{d u}{d v}\,\frac{d\Omega}{u^2\, d v} - \sigma\,(1+s^2)^{-\frac{3}{2}}\, 2\int \mathrm{U}\, d v}{1 + 2\int \mathrm{U}\, d v},$$

but, by an error which is implicitly corrected, the σ which multiplies $(1+s^2)^{-\frac{3}{2}}\, 2\int \mathrm{U}\, d v$ is omitted. Hence the equation (6) becomes

$$\left(1 + 2\int \mathrm{U}\, d v\right)\left(\frac{d^2 \delta u}{d v^2} + \delta u\right) =$$

$$\frac{\mathrm{a}}{\sigma\,\mathrm{a}_{,}\,\psi(e,\gamma)}\left\{\Omega_2 - \frac{d u}{d v}\,\frac{d\Omega}{u^2\, d v} - \sigma\,(1+s^2)^{-\frac{3}{2}}\, 2\int \mathrm{U}\, d v\right\}$$

$$+ \left(1 + 2\int \mathrm{U}\, d v\right) f(e,\gamma)\,\mathrm{Q}'\,e \cos\left(c v - \int \varpi\, d v\right)$$

$$- \left(1 + 2\int \mathrm{U}\, d v\right)\left\{ f(e,\gamma)\,(1+\gamma^2)\,(1+s_{,}^2)^{-\frac{3}{2}} - \frac{\mathrm{a}\,(1+s^2)^{-\frac{3}{2}}}{\mathrm{a}_{,}\,\psi(e,\gamma)}\right\}$$

$$+ \left(1 + 2\int \mathrm{U}\, d v\right) f(e,\gamma)\,\mathrm{P}\,\gamma^2\,(1+s_{,}^2)^{-\frac{3}{2}}\,\Theta,$$

in which equation

$$\Omega_2 = \frac{d\Omega}{d u} + \frac{s}{u}\,\frac{d\Omega}{d s},\ \text{pp. 26, 245},\quad \frac{d\Omega}{u^2\, d v} = \frac{\sigma\,\mathrm{a}_{,}}{\lambda^{\frac{3}{2}}\,(1+\gamma^2)^{\frac{3}{2}}}\,\mathrm{U},\ \text{p. 265},$$

$$\psi(e,\gamma) = \lambda^{-\frac{3}{2}}\,(1+\gamma^2)^{-\frac{3}{2}},\ f(e,\gamma) = \lambda^{\frac{3}{2}}\,(1+\gamma^2)^{\frac{3}{2}},\ \text{p. 261}.$$

But retaining for greater convenience the function $f(e,\gamma)$ in two of the terms, we have

$$(1+2\int \mathrm{U}\,dv)\left(\frac{d^2\delta u}{dv^2}+\delta u\right)=\frac{a}{\sigma a_{,}}\lambda^{\frac{3}{2}}(1+\gamma^2)^{\frac{1}{2}}\left\{\frac{d\Omega}{du}+\frac{s}{u}\frac{d\Omega}{ds}\right.$$

$$\left.-\frac{\sigma a_{,}}{\lambda^{\frac{3}{2}}(1+\gamma^2)^{\frac{1}{2}}}\mathrm{U}\frac{du}{dv}-\sigma(1+s^2)^{-\frac{3}{2}}2\int \mathrm{U}\,dv\right\}$$

$$+(1+2\int \mathrm{U}\,dv)\,f(e,\gamma)\,\mathrm{Q}'\,e\cos\left(cv-\int \varpi\,dv\right)$$

$$-(1+2\int \mathrm{U}\,dv)\,\lambda^{\frac{3}{2}}(1+\gamma^2)^{\frac{1}{2}}\left\{(1+s_{,}^2)^{-\frac{3}{2}}-\frac{a}{a_{,}}(1+s^2)^{-\frac{3}{2}}\right\}$$

$$+(1+2\int \mathrm{U}\,dv)\,f(e,\gamma)\,\mathrm{P}\,\gamma^2(1+s_{,}^2)^{-\frac{3}{2}}\Theta$$

$$=\frac{a}{\sigma a_{,}}\lambda^{\frac{3}{2}}(1+\gamma^2)^{\frac{1}{2}}\left(\frac{d\Omega}{du}+\frac{s}{u}\frac{d\Omega}{ds}\right)$$

$$-a\,\mathrm{U}\frac{du}{dv}$$

$$-\frac{a}{a_{,}}\lambda^{\frac{3}{2}}(1+\gamma^2)^{\frac{1}{2}}(1+s^2)^{-\frac{3}{2}}2\int \mathrm{U}\,dv$$

$$+(1+2\int \mathrm{U}\,dv)\,f(e,\gamma)\,\mathrm{Q}'\,e\cos\left(cv-\int \varpi\,dv\right)$$

$$-(1+2\int \mathrm{U}\,dv)\,\lambda^{\frac{3}{2}}(1+\gamma^2)^{\frac{1}{2}}\left\{(1+s_{,}^2)^{-\frac{3}{2}}-\frac{a}{a_{,}}(1+s^2)^{-\frac{3}{2}}\right\}$$

$$+(1+2\int \mathrm{U}\,dv)\,f(e,\gamma)\,\mathrm{P}\,\gamma^2(1+s_{,}^2)^{-\frac{3}{2}}\Theta.$$

which is

$$=\frac{a}{\sigma a_{,}}\lambda^{\frac{2}{3}}(1+\gamma^2)^{\frac{4}{3}}\left(\frac{d\Omega}{du}+\frac{s}{u}\frac{d\Omega}{dv}\right)$$

$$-a\,\mathrm{U}\frac{du}{dv}$$

$$-\frac{a}{a_{,}}\lambda^{\frac{2}{3}}(1+\gamma^2)^{\frac{4}{3}}(1+s^2)^{-\frac{3}{2}}2\int \mathrm{U}\,dv \text{ (destroyed by term } \textit{infrà})$$

$$+(1+2\int \mathrm{U}\,dv)\,f(e,\gamma)\,\mathrm{Q}'\,e\cos\left(cv-\int \varpi\,dv\right)$$

$$- \lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{4}{3}} \left\{ (1 + s_{,}^2)^{-\frac{3}{2}} - \frac{a}{a_{,}} (1 + s^2)^{-\frac{3}{2}} \right\}$$

$$- \lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{4}{3}} (1 + s_{,}^2)^{-\frac{3}{2}} 2 \int U \, dv$$

$$+ \frac{a}{a_{,}} \lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{4}{3}} (1 + s^2)^{-\frac{3}{2}} 2 \int U \, dv \text{ (destroyed by term } \textit{supra}\text{)}$$

$$+ (1 + 2 \int U \, dv) f(e, \gamma) P \gamma^2 (1 + s_{,}^2)^{-\frac{3}{2}} \Theta;$$

or, putting $u = \frac{1}{a} (u_{,} + \delta u)$, this becomes

$$(1 + 2 \int U \, dv) \left(\frac{d^2 \delta u}{dv^2} + \delta u \right)$$

$$= \frac{a}{e a_{,}} \lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{4}{3}} \left(\frac{d\Omega}{du} + \frac{s}{u} \frac{d\Omega}{dv} \right)$$

$$- U \left(\frac{du_{,}}{dv} + \frac{d\delta u}{dv} \right)$$

$$- \lambda^{\frac{3}{2}} (1 + \gamma)^{\frac{4}{3}} (1 + s_{,}^2)^{-\frac{3}{2}} 2 \int U \, dv$$

$$+ (1 + 2 \int U \, dv) f(e, \gamma) Q' e \cos (cv - \int \varpi \, dv)$$

$$- \lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{4}{3}} \left\{ (1 + s_{,}^2)^{-\frac{3}{2}} - \frac{a}{a_{,}} (1 + s^2)^{-\frac{3}{2}} \right\}$$

$$+ (1 + 2 \int U \, dv) f(e, \gamma) P \gamma^2 (1 + s_{,}^2)^{-\frac{3}{2}} \Theta,$$

agreeing with the Formula II. p. 265, except that in Plana's last term, instead of the factor $f(e, \gamma)$ $\left(= \lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{1}{3}}\right)$, we have the factor $\lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{4}{3}}$. That is, the last term, as given by Plana, should be divided by $1 + \gamma^2$. And this error is introduced from the formula (II.) into the formula (II.)′, p. 267, viz., the incorrect factor $\lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{4}{3}}$ is there replaced by its value q; whereas, the true value being $\lambda^{\frac{3}{2}} (1 + \gamma^2)^{\frac{1}{3}}$, the factor in (II.)′ should be $= \frac{q}{1 + \gamma^2}$.

The corrected formula (II.)′ is

$$- \frac{d^2 \delta u}{dv^2} - \delta u = - Q' \frac{q e}{1 + \gamma^2} \cos (cv - \int \varpi \, dv)$$

$$+ q \left\{ (1 + s_{,}^2)^{-\frac{1}{2}} - \frac{s}{s_{,}} (1 + s^2)^{-\frac{1}{2}} \right\}$$

$$+ \mu^2 (R_1 + R_2) - \mu^2 \left(\frac{du_{,}}{dv} + \frac{d\delta u}{dv} \right) R_{,}$$

$$- 2\mu^2 \left\{ \frac{d^2 \delta u}{dv^2} + \delta u + q (1 + s_{,}^2)^{-\frac{1}{2}} \right.$$

$$\left. - \frac{Q' q e}{1 + \gamma^2} \cos \left(cv - \int w \, dv \right) \right\} \int R_{,} dv$$

$$- P q \gamma^2 (1 + \gamma^2)^{-1} (1 + s_{,}^2)^{-\frac{1}{2}} \left(1 - 2\mu^2 \int R_{,} dv\right) \left\{ \frac{3}{8} \gamma^2 \right.$$

$$\left. - \left(1 + \frac{1}{2} \gamma^2\right) \cos \left(2gv - 2 \int \theta \, dv\right) + \frac{1}{8} \gamma^2 \cos \left(4gv - 4 \int \theta \, dv\right) \right\}.$$

Observing that P is of the order m^2, and that q is approximately equal to unity, the error in $\frac{d^2 \delta u}{dv^2} + \delta u$ is of the order $m^2 \gamma^4$, as noticed above. It may be right to mention that I obtained the correction in the first instance by starting from the fundamental equations, and not as here from the intermediate equation (6), so that there is not in that equation any error afterwards implicitly corrected in the transformation to (II)'.

Jupiter's Satellites: Are they visible to the naked Eye?

By C. Mason, Esq.

Last evening, on seeing *Jupiter* shining with peculiar brightness in a stretch of sky perfectly clear for some distance around him, I determined upon trying the experiment suggested by one of the Secretaries at the last meeting of the Society,—that of looking for the Satellites of *Jupiter* with the unassisted eye.

On steadying my gaze on the planet, I noted that the rays or streamers surrounding it were five in number, each of them distinct and clearly defined. They were placed nearly as shown in the accompanying rough sketch (see figure A), the widest space or interval between any two points being on the western side of the planet, or to my right hand.

After steadily gazing until I had slowly counted twenty, I suspected flashes of a minute luminous point of blue colour, on the western point of the planet, in the space before mentioned. By the time I had counted thirty the point became fixed and

intensely brilliant. Its position was slightly above an imaginary line drawn parallel to the horizon, through the centre of the

blaze of the planet, and distant rather more than one apparent diameter from *Jupiter's* limb.

Three times successively I withdrew my eyes from the planet, and again, on each occasion, picked up the little star, and always found it in the same position. (See same figure.)

The time of the last observation was 10·14 P.M., G.M.T.; the whole of the observations, from first to last, occupying probably from eight to ten minutes.

I should mention that I applied a kind of test of what I saw, or thought I saw, by inclining my head first to the right and then to the left hand. On these occasions I noticed that the rays or streamers attached to the planet moved round its diameter in the same direction as I moved my head; but the little star was a fixture in its position, and remained visible until, by inclining my head further to the right, the nearest ray or streamer came over and obscured it, the ray having moved round the planet towards the west. The same obscuration took place by the south-western ray, which travelled westward round the planet when I inclined my head towards the east.

On reaching my residence, I first made a sketch of the appearances I have attempted to describe, and then, at 11·15 P.M., turned my refractor, of $4\frac{7}{8}$-inches aperture and 6 ft. 6 in. focal length, upon *Jupiter*, and observed that the whole of the satellites were on the western side, and placed somewhat thus (see figure B).

In the same field, and near the Satellites, were two minute fixed stars, shown also in the sketch.

Until I had referred to the *Nautical Almanac*, a few

minutes before using the telescope, I had not the slightest conception of the relative positions of the Planet and its Satellites for that evening. I therefore am the more strongly impressed with a conviction of having seen the Satellites with the naked eye.

16 *Queen's Road, Regent's Park,*
April 16th, 1863.

Phenomena of Jupiter's Satellites and Occultations of Stars by the Moon. Observed by S. Gorton, Esq.

The accompanying observations were taken more with a view of recording unusual phenomena than for the purpose of giving the absolute time of the occurrence, although I believe that to be approximately correct.

As it has been frequently noticed that the stars reappear after being occulted, I have been anxious to observe whether such an occurrence would take place when positively watched for. With the exception of very great "oscillation" on the part of *Jupiter's* Second Satellite, on the 26th ult., I did not perceive anything unusual.

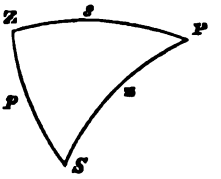
Jupiter's Satellites: Occultations of Stars by the Moon: observed at Downs Road, Clapton.

Approximate Longitude, 14° W.

1863.		Local Sid. Time. h	m	s
April 19	Eclipse: Reappearance of Jupiter's 2d Satellite ...	11	5	9
	(The Satellite was two seconds or more in reappearing; the time therefore is not correct to the exact second.)			
26	Occultation of Jupiter's 2d Satellite:			
	Satellite in contact	10	56	0
	,, bisected	11	0	0
	,, 3 parts covered ...	11	2	0
	,, disappeared at ...	11	3	0
	(The Occultation occupied nearly seven minutes, during which time, owing apparently to the movement of the atmosphere, the Satellite seemed to disappear and appear again several times.)			
26	The Star *s* Cancri was occulted by the Moon at ...	12	1	30
29	The Star *e* Leonis was occulted at	12	57	4
	(In both cases the Occultation took place at the Moon's dark limb, and was sharp and instantaneous.)			

Telescope 4 feet focal length; $3\frac{1}{8}$ inches aperture.

On the Solution of some Problems in Nautical Astronomy. By Major-General Hannyngton, late of the Bengal Army.



In the spherical triangle Z P S;

$$\cos P = \frac{\cos p - \cos z \cos s}{\sin z \sin s}$$

$$= \cot z \cot s \,(1 - \sec z \sec s \cos p), \qquad (1)$$

also,

$$\cos p = \cos z \cos s + \sin z \sin s \cos P$$

$$= \cos z \cos s \,(1 + \tan z \tan s \cos P), \qquad (2)$$

and moreover

$$\frac{\sin z}{\sin Z} = \frac{\sin p}{\sin P} = \frac{\sin s}{\sin S}. \qquad (3)$$

The equations marked (1) and (2) are merely transformations. Their symmetry is remarkable, and they appear to be well suited for use in the only two cases that occur in practice, namely,—

(1.) Having three sides, to find the angle opposite to one of them.

(2.) Having two sides and the angle between them, to find the third side.

In either case, the other parts may be found by means of formulæ (3).

If P be the pole, Z the zenith, S the sun or a star, then the angles at those points may be called those of Time, Azimuth, and Position; also, the sides p, z, s, will represent Zenith distance, Polar distance, and Co-latitude.

It is further to be noted, that if a side or an angle exceed 90°, then the affection of the cosine, secant, tangent, and co-tangent of that side or angle is supposed to change and become negative. The cases in which this occurs must be interpreted accordingly.

Formula (1) is applicable to the finding of the Hour-angle, or the Azimuth of a heavenly body, at the instant when its Altitude was observed. By aid of formula (3), a slightly extended process will determine both of these at one operation.

Formula (2) will serve to determine the Altitude of a [illegible]n object when its Hour-angle is known, or the distance betwe[illegible]n two objects whose Right Ascension and Declination are know[illegible]n. It is, therefore, applicable to the clearing of a Lunar Distance, or to the finding of the Latitude by double altitudes, or to the find[illegible]ng of distances on a great circle.

In the following examples* the *observed* quantities [illegible]ill

* Three only of the nine examples given by the author are printed.—— Ed

generally be used, instead of the sides, to which they are complementary. When a side or angle appears as an intermediate result, any further tabular entries depending upon it will be named accordingly, so that in all cases the quantities in hand will be used unchanged. The data will be taken from Raper's *Navigation*, sixth edition.

Formula (1).

Ex. 1. Alt. 37° 51′; Lat. 51° 10′ N.; Decl. 19° 27′ N.

Find the hour-angle. (Raper, page 202.)

Alt.	37° 51′	sin	9·78788		
Lat. N.	51 10	cosec	0·10848	tan	0·09422
Decl. N.	19 27	cosec	0·47758	tan	9·54794
		log x	0·37394	log $(x-1)$	0·13532
Hour-angle	$3^h\ 32^m\ 47^s$			cos	9·77748

Here $x = 2·3656$, and the sides are each less than 90°, therefore the log of $(x - 1)$, *i.e.*, the log of 1·3656 is added to the log tangents. It will be observed that the latitude and declination are of the same name.

Formulæ (1) *and* (2) *combined.*

Clearing a lunar distance.

Ex. 7. App. Alt. ☉ 47° 31′; True Alt. 47° 30′ 13″
App. Alt. ☽ 36 52; True Alt. 37 37 35
App. Distance 48° 20′ 29″

Find the true distance. (Raper, page 289).

App. Dist.	48° 20′ 29″	cos 9·82261					
App. Alt. ☉	47 31	cos 0·13225		tan	0·03820		
App. Alt. ☽	36 52	cos 0·22182		tan	9·87501		
		log x 0·17674	log $(x-1)$		9·70091		
True Alt. ☉	47 30 13			cot	9·96200	sin	9·86766
True Alt. ☽	37 37 35			cot	0·11308	sin	9·78569
			log y		9·68920	log $(y+1)$	0·17286
True Dist.	47 55 00					cos	9·82621

In the first part of this example, $x = 1·50224$, and $(x - 1) = 0·50224$; and in the second part, $y = 0·48888$, and

$(y + 1) = 1{\cdot}48888$. The latter part is in effect the converse of the former part, with the true altitudes substituted for the apparent altitudes. As $(x + 1)$ was used in the former part, so $(y - 1)$ is used in the latter part.

Formulæ (1), (2), *and* (3), *combined.*

Latitude by Double Altitudes.

Ex. 9.	Markab R.A.	22 h 57 m 44 s,	Decl. 14° 27′ N.,	True Alt. 58° 55′
	Altair R.A.	19 43 53,	Decl. 8 30 N.,	True Alt. 23 30

Find the latitude and the hour-angles. (Raper, page 255.)

Polar Angle		$3^h\ 13^m\ 51^s$	cos 9·82159				sin 9·87420
Decl.	Markab	14° 27′	cot 0·58891	sin	9·39713		
	Altair	8 30	cot 0·82550	sin	9·16970		cos 9·99520
	$x = 17{\cdot}219$		log x 1·23600	log $(x + 1)$	1·26052		
	Distance	47 46¾		cos	9·82735		cos 0·13044
	A	88 27					sin 9·99924
Alt.	Altair	23 30	sin 9·60070				
	Markab	58 55	cos 0·06731	tan	0·21980		
	Distance	47 46¾	sec 0·17264	cot	9·95780		
	$y = 0{\cdot}69287$		log y 9·84065	log $(y - 1)$	9·42732		
	B	62 28		cos	9·66492		
	(A − B)	25 59	cos 9·95372				sin 9·64158
Markab	Decl.	14 27	cot 0·58891	sin	9·39713		
	Alt.	58 55	cot 9·78020	sin	9·93269		cos 9·71289
	$z = 2{\cdot}1030$		log z 0·32283	log $(z + 1)$	0·49178		
	Latitude	41 32½ N.		sin	9·82160		sec 0·12582
	Hour-angle of Markab		$1^h\ 10^m\ 21^s$				sin 9·42029
Altair Alt.		23 30	sin 9·60070				
	Lat. N.	41 33	cos 0·17831	tan	9·94757		
	Decl. N.	8 30	cos 0·83030	tan	9·17450		
	$x =$		log x 0·60931	log $(x - 1)$	0·48676		
	Hour-angle of Altair		$4^h\ 24^m\ 7^s$	cos	9·60883		

The difference of these hour-angles $3^h\ 13^m\ 46^s$, differs fr[illegible] the polar angle by 5^s only. The computed latitude may th[illegible]fore be relied on. It may be observed that the latitude [illegible] account is not an element of our calculation.

The angle marked A, is the angle at *Markab*, subtend[illegible]

the arc between *Altair* and the Pole; and the angle marked B, is the angle at *Markab*, subtended by the arc between *Altair* and the Zenith. The difference of these (A — B) is the angle of position of *Markab* at the time of the observation. We might in this case, with equal correctness, have determined the latitude by means of the corresponding angles at *Altair*.

The methods herein shown may deserve attention. A little practice would render them familiar, and they require no special tables. Printed forms would also assist the memory and reduce the amount of work.

Trigonometric tables should always have Time arguments. Dr. Inman's, to every second of time, is, for nautical use, extremely convenient. Tables of log $(1 + x)$ and log $(1 - x)$, such as those of Mr. Gray, might advantageously be applied to the foregoing processes.

Occultation of κ Cancri, observed at Highbury. By T. W. Burr, Esq.

1863, April 26th. The star κ *Cancri* disappeared instantaneously at the dark limb of the Moon at $12^h\ 1^m\ 18^s$, corrected Local Sidereal Time. The reappearance was not observed. Telescope 4 ft. 4 in. focus, $3\frac{1}{8}$ inches aperture. Power 123. Longitude, 24^s W.

Highbury, May 5th, 1863.

Remarkable Phenomenon attending the Lunar Occultation of κ Cancri, on the 26th April, 1863. By the Rev. W. R. Dawes.

The disappearance of κ *Cancri* at the Moon's dark limb, on the evening of the 26th ult., was observed at West Gorton, near Manchester, by Mr. Ralph Copeland, with a 5-inch refractor by Messrs. T. Cooke and Sons; and the next day the following account of the observation was communicated to me by the observer: —

"The disappearance was very remarkable. About three-fourths of the light disappeared in the usual instantaneous manner; and after an interval of (as near as I could judge) rather more than half a second, the remaining portion disappeared. I did not observe the reappearance."

In a subsequent letter Mr. Copeland says:—"I was so surprised at the totally unexpected phenomenon, that I cannot give the *time*" [*i. e.* the interval between the partial and the total disappearance] "great weight; but I am certain that it was less than one second."

As such a phenomenon points distinctly to the duplicity of the star occulted, I have carefully examined it, under the best circumstances which have since occurred, with my 8¼-inch refractor. In hopes that the diminution of the disks by strong twilight might enable me to detect a separation of the components which might not be discernible in a dark sky, on May 1st I set my telescope upon it so early as $6^h\ 22^m$ G.M.T., the star being about five minutes east of the meridian. The sun was shining brightly; but the star (of the fifth magnitude) bore sufficiently well a power of 620, and occasionally the disk was tolerably free from tremour and moulding; yet no separation, or even elongation, could be perceived. Subsequently a power of 1000 was applied, with which the disk was at times well seen, but under the best vision it was perfectly round. The observations have since been repeated, though under circumstances scarcely so favourable, but with the same negative result. It would however, I think, be highly desirable that the star should be examined under the best atmospheric conditions, and with refractors of larger size than my own, of which, happily, there are already several in this country.

Two more occultations of κ *Cancri* will occur during the present year, and three in 1864, visible at Greenwich.

As the occultation on April 26th occurred at a convenient time, it may probably have been observed elsewhere; and if so, it would be interesting to know whether any unusual phenomenon was noticed.

Hopefield Observatory, Haddenham, Thame,
May 6, 1863.

Some Observations of the Lunar Crater Plato.
By the Rev. W. R. Dawes.

Having been requested by Mr. Birt, who has been so diligently engaged in scrutinising the Lunar Crater termed *Plato*, to examine the floor of that crater with the more powerful means at my disposal, I took advantage of a rather favourable night for that purpose immediately succeeding the receipt of his letter on January 31st; and as some of the points I then made out were, so far as I know, previously unobserved, and subsequent observations have confirmed and extended the discoveries then made, Mr. Birt has requested

me to put the results on record by communicating them to the Society.

Three crater-like spots had previously been noted on the floor of *Plato;* one near the centre, which is the largest, and visible with very moderate optical aid; another toward the south-east; and the third to the north and a little east of the central spot. On the evening above mentioned (Jan. 31st) I noticed that with power 105 this small northern spot was decidedly elongated in an oblique direction, namely, north-east and south-west. On raising the power to 209 I instantly perceived that this apparent elongation arose from the spot being actually *double*, being composed of two very small round crater-like spots, nearly equal in size and brightness, and about one diameter of either apart. All this was seen much plainer with powers from 300 to 620. But whether their circular margins were actually raised above the common level, or whether their interior was depressed below it, I could not determine in the sunlight which then fell upon that part of the moon, her age being about 12·2 days. But on the 26th of April, from $7^h\ 52^m$ to $8^h\ 38^m$, G.M.T., I was so fortunate as to catch a good view of *Plato* as soon as the shadows of the western mountainous ridge were withdrawn from the central spot and the small double one to the north, and while the long spire-like shadow of the fine peak on the western ridge was yet stretching completely across the south-eastern portion of the floor; the northern edge of that shadow actually bisecting the small crater situated in that part. Under these circumstances I was gratified to find that the true character of all these spots was fully brought out, and they were proved to be veritable craters. The eastern wall of the central crater cast a decided shadow on the outside, and the western wall cast a small shadow inwards. Precisely the same appearance was visible in each of the components of the double spot, the length of the exterior shadow of each being, as nearly as I could judge, equal to the diameter of either of them. The appearance of the small crater to the south-east was very similar, but was at first interfered with by the edge of the spire-like shadow which rendered it difficult to be certain of all the details. At $10^h\ 52^m$ G.M.T., however, the apex of this shadow exactly reached the western wall of the crater, and I was then able to make out features of light and shade precisely like the others, but only by transient glimpses, the Moon being then too near the horizon to bear well sufficient power.

There are many other bright spots and streaks on the floor of *Plato;* but the streaks seem to resemble those which extend from some of the principal craters, and show neither elevation above nor depression below the surface over which they pass; and the brighter patches appear to be merely suddenly enlarged portions of these streaks, being irregular in

outline and destitute of elevation. They are all best seen when the Sun's rays fall nearly vertically upon them; while under very oblique illumination they can scarcely be seen at all.

There are two other extremely small spots, one a little to the south-east of the central crater, the other nearly due east of it, which *may*, from their sharp definition and round form, probably be craters; but they require good circumstances and high powers to see them at all; and I could not, on the 26th ult., discern the slightest shadow from either of them.

Hopefield Observatory, Haddenham, Thame,
May 7, 1863.

On a Portion of the Surface of the Mare Imbrium, observed with the Hartwell Equatoreal. By W. R. Birt, Esq.

The surfaces of most of the so-called lunar seas are characteristically different from all other portions of the lunar surface. A general absence of ruggedness, but few craters, and those only thinly scattered, soft undulating ridges bearing apparently some relation to the borders of the *Maria*, are (with the smaller quantity of light reflected from them) features which at once separate the dusky portions from all others on the lunar hemisphere turned towards the Earth. When the solar light reaches them under a high angle, numerous variations of luminosity are detected, round spots of light that before were invisible manifest themselves, streaks seen to radiate from central craters cross them in various directions, the result being that under a mid-day illumination most of the *Maria* present a well-known mottled appearance, which is scarcely, if at all, observable under either the morning or evening illuminations.

The object of this communication is to place on record a feature by no means so common as any alluded to above. It is a deepening of the tint of the surface, accompanied by a *purity* not to be found in the speckled and mottled surfaces of the *Maria;* and the one I am about to describe appears to be intimately associated with the range of mountains known as the *Alps*, and the bright grounds south and east of *Plato*.

On the evenings of May 2 and 4, 1863, the illumination was sufficiently advanced to bring out, with admirable precision, this deeper tint on the surface of the *Mare Imbrium*. I have thus recorded its appearance at the time of observation: "Bordering the *Alps* from the most extreme end towards the south-west, as far as *Plato*, is a portion of the *Mare Imbrium* of a *pure* dark colour. Its general contour is more or less parallel with the *Alps* and the bright ground which

extends in a gentle curve from the *Alps* to *Plato*. Where this border is not interrupted by streaks or spots, the colour is very pure. In the neighbourhood of the south-east extremity of the wedge-shaped valley of the *Alps* this pure colour extends outwards, so that there is a greater space of purer colour absolutely existing at what may be called the estuary of the valley, as if the convulsion which had produced the valley had interfered with the nature of the surface hereabouts. Just preceding this larger extension of the purer colour the greatest interruption of the border occurs in consequence of some rays which proceed from *Aristillus*, the rays extending nearly across the smoother and darker border. The extension of this pure tint of surface in the opening of the valley is curious, opening out as far into the *Mare Imbrium* as the mouth of the valley indents the general border of the *Alps*. Just eastward of the mouth of the valley this pure border is again interrupted by a lighter portion of the surface, on which stand two small craters, close together, marked K by Beer and Mädler. These craters are slightly removed, towards the east, from a line coincident with the direction of the wedge-shaped valley. Proceeding eastward, this border of a deeper and purer colour meets with no further interruption until it terminates south of *Plato*. It appears to take the general form of the bright ground north of it, but widens out a little towards *Pica*.

"The south-east ascent to the summit of the ring of *Plato* completely obliterates this delicate border. Eastward of the south-east ascent it appears again much contracted towards its eastern end, in close proximity with the bright ground extending from *Plato* to the promontory of *Laplace*, and terminating in a fine point north of the middle of a remarkably straight chain of mountains between *Plato* and *Laplace*. It has considerable breadth where it abuts on the brighter ground of the south-east ascent to *Plato*."

This border, which is somewhat indistinctly shown on De la Rue's photographs, is best seen a little before or a little after full moon; good definition is requisite to bring it out well, and the eye should be protected by a slightly-tinted glass.

Elements of the Comets II. and III., 1863.
By Mr. Hermann Romberg.

(*Communicated by J. Gurney Barclay, Esq.*)

Both Comets now visible have been frequently observed here, but the reductions are not yet completed. Observations

may probably be continued till the end of the month. By a preliminary calculation on three Leyton observations the following elements have been obtained:—

Comet II. 1863.

(Discovered by Dr. Klinkerfues.)

T 1863, April 5·04696 Greenwich M.T.

π	247° 7′ 42″·3	Apparent Equinox of April 10.
☊	251 19 32·4	
i	67 13 38·0	
log q	0·028804	

Retrograde.

The errors for the middle observation being only

$$\Delta\alpha = +1''\cdot7 \qquad \Delta\delta = +2''\cdot6 \quad (C. - O.)$$

These elements agree very closely with M. Löwy's, published in the Paris *Bulletin* for April 30. The Comet's appearance is that of a round nebula strongly condensed in the middle, and having a very faint nucleus. It was easily observed during moonlight.

Comet III. 1863.

(Discovered by Prof. Respighi at Bologna.)

For this Comet I have calculated from one Berlin and two Leyton observations the following elements:—

T 1863, April 22·33050 Greenwich M.T.

π	296° 53′ 49″·3	Apparent Equinox of April 20.
☊	241 14 35·8	
i	84 36 18·9	
log q	9·788234	

Direct.

The appearance of this Comet is much more interesting than that of the second: for the week following April 19 it was a very brilliant little object with low powers on the refractor. It is now visible during the whole night near the northern horizon, and may be observed on the meridian at its transit *sub polo*.

Mr. Barclay's Observatory, Leyton,
May 6.

Observations of the Comets II. and III., 1863.
By R. Hodgson, Esq.

Comet II. 1863.

1863.	G.M.T. h m s	R.A. h m s	Decl. ° ′
April 20	12 2 13	20 27 0	+15 42 ±

Apparent size about 4′ of arc, with a bright condensed nucleus.

May 1	13 1 0	19 57 54	+50 8 ±

Less bright than on April 20.

4	11 40 0	19 42 38	+57 57 ±

Fast diminishing in brightness.

Comet III. 1863.

1863.	G.M.T. h m s	R.A. h m s	Decl. ° ′
April 26	13 50 45	0 33 11	+42 1 ±

A beautiful object, very brilliant, and a miniature of Donati's Comet; decided stellar nucleus, and tail of at least a degree in length; visible to the naked eye.

	G.M.T. h m s	h m s	° ′
May 7	14 0 0	2 3 28	+47 35 ±

Much less brilliant, and not observable with the naked eye.

Grove House, May 8, 1863.

Observations and Parabolic Elements of Comet III. 1863.
By M. Karlinski.

1863.	Cracow M.T. h m s	Comet's App. R.A. h m s	Log $\left(\frac{P}{\Delta}\right)$.	Comet's App. Decl. ° ′ ″	Log $\left(\frac{P}{\Delta}\right)$.
April 18	15 5 54·7	23 25 53·18	9·6264	+30 44 18·9	0·7803
20	14 3 1·5	23 41 34·26	9·6005	34 3 28·6	0·8308
„	14 28 11·6	23 41 42·60	9·6208	34 5 13·3	0·8104
21	14 26 44·9	23 79 57·95	9·6249	35 37 13·1	0·8114
23	14 3 26·4	0 6 44·31	9·6116	38 21 19·1	0·8326
May 2	14 18 18·1	1 24 7·05	9·6299	45 48 47·2	0·8377
4	13 32 59·2	1 39 47·07	9·5411	46 34 31·4	0·8772
5	13 41 50·2	1 47 38·05	9·5553	+46 52 3·7	0·8730

Parabolic Elements, computed with regard to Parallax and Aberration, from the observations of 18, 20, and 23 April, are

$$T = 1863, \text{ April } 20{\cdot}89771 \text{ Berlin M.T.}$$

$$\left.\begin{array}{lcl} \pi & = & 305^\circ\ 31'\ 5''{\cdot}6 \\ \Omega & = & 279\ 59\ 21{\cdot}7 \\ i & = & 85\ 28\ 73{\cdot}5 \end{array}\right\} \text{Mean Equinox } 1863{\cdot}0$$

$$\log q = 9{\cdot}798266$$

Motion direct.

Cracow, May 6, 1863.

RECENT PUBLICATIONS.

Observations de la Grande Nébuleuse d'Orion faites à Cazan et à Poulkova. 1re Partie. *Mémoire de M. Liapounov sur les Observations de Cazan.* 2e Partie. *Additions au Mémoire de M. Liapounov, et Observations de Poulkova.* (*Avec* 4 *planches*). 4to. St. Pét. 1862, pp. 1–122; forms part of t. v. of the *Mémoires de l'Académie de St. Pétersbourg*, viie Série.

The chief object of M. Liapounov's Memoir is the construction of a Catalogue of the exact positions of the visible stars of the Nebula, to serve as a point of departure for ulterior researches on the nature of this magnificent star — researches for which a solid basis was given by the labours of Sir John Herschel at the Cape. In the course of his work the author has collected some detached observations, made under favourable atmospheric circumstances, on the regions of the Nebula most accessible by their brilliancy to the power of his telescope; and the results of these observations are given at the end of the Memoir. He remarks, however, that, under the less favourable conditions of his station, such researches presented great difficulties, and that, for this reason, he considers the last-mentioned results as only provisionally established.

The observations were continued regularly in the years 1847–1851, with a single break in the year 1850. The space surveyed, in the neighbourhood of *θ Orionis*, was about 0·12 of a square degree; whereas the telescope of Sir J. Herschel gave to the Nebula an extent almost double, 0·22 of a square degree.

The stars made out in the space indicated were at first 56 in number; this number was increased to 78 by the successive discovery of some more feeble stars. Five more stars,

situate a little beyond the limits mentioned, were afterwards added, making together 83 stars the relative positions of which were to be determined by micrometric measures; but some other stars were observed, making in all 125 out of the 150 stars of Sir J. Herschel; and besides two stars, one of the 9th magnitude, probably excluded by Herschel on account of its too northerly position, the other a star of the 12th magnitude, which is wanting also in the catalogue of Prof. Bond. The Catalogue of the definitive positions in regard to *θ Orionis* of the other 82 stars, is given (pp. 61–63); and the results are compared with those of Herschel and Bond, and of M. Lamont. The concluding part is occupied with physical observations of the Nebula, a drawing of which is given in Plate III.; but it is remarked that the artist has found much difficulty in suitably expressing the relative intensities of the different regions.

The second part, by M. Otto Struve, is in a great measure supplementary to the other. It contains a comparison of M. Liapounov's results with those of Mr. Lassell, obtained by him at Malta in the years 1852 and 1853 (*Mem. R. Ast. Soc.*, t. xxiii.), and a supplementary comparison with the results of Herschel, Bond, and Lamont, and with some other ancient and modern observations. It contains, also, M. Struve's own observations at Poulkova in the years 1856–1862, and, in conclusion, a single Catalogue of the positions in respect to *θ Orionis* of all the stars the existence whereof has been verified with the Refractor at Pulkova. The Catalogue contains, in the order of their Right Ascensions, all the 150 stars of Herschel and 5 supplementary stars. For all the 82 stars measured by Liapounov, the positions are borrowed from his Catalogue; 59 others are introduced, according to the measures and estimations of Herschel, 3 from Lassell's Catalogue, and 1 from that of Bond; for 10 stars the positions are given, such as they were determined at Pulkova. The Plate IV. gives a graphical representation of the relative positions of the stars in the Catalogue, sufficiently accurate to serve for comparison with the heavens, or for the construction, by its aid, of exact drawings of the Nebula.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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C. B. VIGNOLES, Esq., Vice-President, in the Chair.

The Ven. Archdeacon Garbett, Chichester, was balloted for and duly elected a Fellow of the Society.

On the Indications by Phenomena of Atmospheres to the Sun, Moon, and Planets. By Professor Challis.

As the Earth has an atmosphere, it is not unlikely that the other bodies of the solar system are similarly furnished, and the knowledge we have of our own atmosphere may reasonably be employed in inquiring whether the existence of other atmospheres of the same kind is indicated by phenomena. I propose, therefore, in this communication to discuss generally the possible effects, observable from a distant position, of an atmosphere constituted like the Earth's, with the view of ascertaining whether such effects have been actually observed.

The most notable effect of the Earth's atmosphere is the refraction of the rays of light which pass through it. In consequence of this refraction, the apparent magnitude of the Earth, as seen by a distant spectator, would be in some degree greater than the apparent magnitude of the globe deprived of its atmosphere, because the rays which pass from the periphery of the globe to the position of the spectator must have a curved course concave to the straight line joining that position and

the Earth's centre. It happens that we are able to calculate exactly the amount of the augmentation in the case of the Earth, independently of any law of variation of the density of the atmospheric strata. I am indebted to Professor Adams for pointing out to me that this inference may be very simply drawn from the construction given by Mr. Sang, in a "Note on the Method of Computing the Moon's Parallax," contained in vol. xv. p. 169, of the *Monthly Notices*. The ratio of E K to E L, which is the ratio of the refractive index for the lowest stratum of the atmosphere to that for the highest, becomes, for the case of a ray which just grazes the Earth's surface, the ratio of E K to E A. Supposing the spectator to be placed at the Moon, this ratio is that of the Moon's horizontal parallax augmented by refraction, to the horizontal parallax unaugmented. As the refractive index for the highest stratum may be taken to be unity, if μ be the refractive index for the lowest stratum, we thus find that the augmentation is equal to $(\mu - 1) \times$ the Moon's horizontal parallax. This quantity is almost exactly $1''$. A mountain five miles high, situated at the Earth's apparent border, would subtend at the Moon an angle of $4\frac{1}{2}''$, and its top would be somewhat less elevated by the refraction than the base, on account of being higher up in the atmosphere. Thus it appears that the atmospheric refraction would only produce the effect of a slight apparent depression of the mountain. These results show that any atmosphere the Moon can be supposed to have would neither augment her apparent diameter to a perceptible amount, nor alter the forms of the inequalities on the periphery.

There is, however, another effect of atmospheric refraction which might determine whether or not a lunar atmosphere exists. We know that, to a spectator on the Earth, the Sun or a star remains visible above the horizon, by the effect of refraction, when its actual direction falls below the horizon. The same thing would happen to a spectator at the Moon. By reason of the bending of the rays in their passage through the Earth's atmosphere, a star about to be occulted would be visible when its actual direction from the place of the spectator falls within the Earth's apparent periphery. In consequence of the rays by which it is seen being successively more and more bent by the refraction, the star will appear to retire before the Earth's limb, and will disappear suddenly when the rays just graze the Earth's surface. Analogous effects would be produced at a reappearance: the star will be first seen to emerge from the limb when its proper direction falls within the Earth's contour, and will then separate from the limb at an increasing rate till it escapes from the influence of the terrestrial refraction. It is evident that if the Earth's apparent diameter were calculated from the observed times of such disappearances and reappearances, without taking any account of the effect of refraction, the result would be much less than the true value.

These considerations show that if the Moon has an atmosphere constituted like the Earth's, her apparent diameter, as inferred from the calculation of occultations of stars and eclipses of the Sun, would be less than the diameter obtained by instrumental measurement, provided the atmosphere produced a sensible amount of refraction. A large number of occultations of stars and planets observed at the Cambridge Observatory, the results of which are exhibited in final equations involving corrections of the quantities assumed in the calculations, might be used for determining this point. It would, however, be necessary in each instance to correct the place of the Moon and that of the star or planet; and, after all, the determination of apparent diameter would be uncertain on account of the liability to error being much greater in observing occultations at bright limbs than in observing those at dark limbs. As far as I have gathered from the mean of a large number of equations, on the supposition that the only error to be corrected is that of the Moon's assumed semidiameter, the result does not appear to confirm the hypothesis of an atmosphere. On the other hand, the Cambridge observations of the Solar Eclipse of March 15, 1858, reduced so as to correct the tabular differences of the R.A. and N.P.D. of the Sun and Moon, give a correction of the Moon's semidiameter in the *Nautical Almanac* equal to $-2''\cdot96$; and Mr. Airy obtained from a discussion of the Cambridge observations of the Solar Eclipse of July 16, 1833, a like correction equal to $-4''\cdot21$. (See *Memoirs of the Royal Astronomical Society*, vol. viii. p. 133, and vol. xxvii. p. 60.)

In an eclipse of the Sun by the Earth, as seen from the Moon, the terrestrial refraction would cause solar rays to reach the Moon, which come from parts of the Sun's disk that are really behind the Earth. There will, consequently, be a condensation of light, so far as this cause operates, bordering the Earth's limb, but extending a very little distance from it, since the whole height of the terrestrial atmosphere, assuming it to be 60 miles, subtends only an angle of $52''$ at the Moon. This light will be much diminished in intensity by having to pass through the dense parts of the Earth's atmosphere, as is evident from the fact that the Sun's face, when near setting, loses so much brightness that it can be looked at with impunity with the naked eye. Also, the rays from this fringe, being divergent, will on that account fall with less intensity on the Moon. But the fringe will remain, and a narrow strip of the Sun's face be apparent long after the disk is geometrically behind the Earth; and the light will generally acquire a ruddy colour by passing, for the most part, through the vapours near the Earth's surface. The red appearance of the Moon's disk during a total lunar eclipse is, as is well known, accounted for in this manner. If we now inquire whether any analogous phenomena are presented in an eclipse of the

Sun by the Moon, we may at once pronounce, considering that if there be a lunar atmosphere, it must be of extreme rarity, that no effect would be at all likely to be discernible, except a slight condensation of solar light bordering the part of the Moon's periphery which crosses the Sun's disk. It is stated by Professor Stephen Alexander, in an account of observations of the total Solar Eclipse of July 18, 1860, published by the Superintendent of the United States Coast Survey, that himself and two other observers saw "a bright band bordering the edge of the Moon which was projected on the Sun's disk." (Appendix, No. 21, to Report for 1860.) It might be doubted from this description whether the band was on the Sun's disk or the Moon's. But, as one of the observers says that he saw "the Sun brighter along the border of the Moon," I conclude that the band was exterior to the Moon's edge. Professor Alexander further states that he first noticed the phenomenon in 1831, and had since, on various occasions, called the attention of others to it; also that "the bright band is unquestionably pictured in the copies of daguerreotypes of the eclipsed Sun." I have referred to these statements in the hope that in future eclipses of the Sun, partial or total, observers may have their attention directed to this appearance, which, as bearing on the question of a lunar atmosphere, is of no little interest.

In the case of an occultation of a star by the Earth, as seen from the Moon, if the strata of the atmosphere were transparent and undisturbed, and everywhere of the same density at the same height above the Earth's surface, the rays by which the star is last or first seen would coincide with rays from the apparent periphery of the Earth, and the star at disappearance or reappearance would, consequently, appear to be exactly on the periphery. But disturbances and variations such as we perceive actually to take place in our atmosphere would destroy this coincidence, and cause the disappearances and reappearances to occur at points separated, on one side or the other, from the Earth's limb. Circumstances of this very kind have been noticed in occultations of stars by the Moon, and would seem to admit of explanation by the hypothesis of a lunar atmosphere. But, although such phenomena, and that above mentioned of the bright band bordering the Moon in a solar eclipse, are reconcileable with the supposition of an atmosphere, they would hardly suffice to prove its existence. An ascertained excess of the Moon's diameter, as measured, above the diameter determined by occultations of stars and solar eclipses, would be a crucial fact, inasmuch as it does not appear that this fact could be accounted for on any other supposition than that of a lunar atmosphere.

I proceed now to consider another effect of atmospheric refraction; in noticing which, as far as I am aware, I have not been anticipated by any previous investigator. Hitherto it has been supposed that a ray from a star or other object may

pass *through* the atmosphere in a course which grazes, or is a tangent to, the interior globe. It is evident that in such a case the curvature of the path of the ray is *less* than the curvature of the surface of the globe. But it is conceivable that there may be such a relation between the gradation of density of the atmospheric strata, and the curvature of the globe, that this condition cannot be fulfilled. For instance, it is reasonable to suppose that this is the case in the Sun's atmosphere, when the vast magnitude of the globe is considered, and the usual equation $-k\,d\varrho = G\varrho\,dz$ is taken into account, which shows that the decrement $-d\varrho$ of density corresponding to a given increment dz of height varies conjointly as ϱ the density and G the Sun's gravity. It will, therefore, be worth while to inquire what optical effects will result under these circumstances.

A ray of light proceeding from a point on the surface of the globe, in a direction making an angle with the surface less than a certain limiting angle (α), will not pass out of the atmosphere, but will either reach a maximum distance and then return by a like path to the surface again, or will be reflected internally from the upper atmospheric surface, if the atmosphere be of finite density at its superior limit. Conversely, a ray entering the atmosphere from an external body could not impinge on the surface of the globe in a direction making with it an angle less than α. It seems necessary to suppose that an atmosphere has an upper boundary like that of an ocean, because, the density continually decreasing with the height, a point must at length be reached at which the upward repulsive force of an atmospheric stratum is just equal to the force of gravity, in which case there can be no downward repulsive force, and therefore no further extension of the atmosphere. Now the limiting angle α is the angle made with the surface of the globe by the course of a ray, which, after traversing the atmosphere and being refracted at the boundary, issues out in the direction of a tangent to the refracting surface. No ray which comes to the eye of a distant spectator can be inclined to the surface of the globe at a less angle than this.

From these considerations it follows (1) that rays proceeding from points of the Sun lying *beyond* the surface which contains all these limiting courses cannot reach the eye of a distant spectator; (2) that rays proceeding from points *within* that surface appear to come from points within the Sun's periphery; (3) that rays proceeding from any points *on* that surface eventually are tangential to the boundary of the atmosphere, and appear to come from the Sun's periphery. Consequently, if the boundary of the atmosphere be spherical, the periphery of the Sun will be an exact circle, notwithstanding any actual inequalities of the surface of the globe. Also, if we may suppose that any objects like clouds are suspended in the solar atmosphere, all the points lying on the limiting

courses will be brought by the refraction to the same level, that is, to the level of the upper boundary of the atmosphere. The augmentation of the Sun's apparent diameter as seen from a distant point will be just equal to the apparent height of the atmosphere.

The foregoing theory fully explains why the contour of the Sun is generally observed to be entirely free from inequalities. This fact is the more remarkable because the unevenness of the Sun's surface, as indicated by the mottled appearance spread over its whole extent, with the occasional occurrence of deep depressions (the spots), surrounded generally by lofty ridges (the faculæ), gives reason to expect that inequalities would be perceptible on the periphery. I am aware that Mr. Dawes has recorded the observation on Oct. 22, 1859, of a phenomenon, which he describes as "a bright streak, which formed the very edge, projecting irregularly beyond the circular contour of the edge." (*Monthly Notices*, vol. xx. p. 56.) This observation, which, as being made by so experienced an observer, must be accepted as trustworthy, by proving the possibility of seeing inequalities, provokes the enquiry, Why are they not more generally seen? The above theory accounts for their not being visible under ordinary circumstances, and at the same time it is possible that the solar atmosphere may be liable, under abnormal conditions, to disturbances and changes of level sufficient to account for the phenomenon Mr. Dawes witnessed. Besides this instance of inequality of contour I am acquainted with only that which was observed on Aug. 4, 1862, by Mr. Howlett, who states that "he plainly perceived a small notch in the Sun's margin, which struck him as not being caused so much by any deficiency in the circular limb of the Sun itself, but rather by reason of abnormally heaped up masses of the contiguous photosphere." (*Monthly Notices*, vol. xxiii. p. 113.) This phenomenon would thus seem to be similar to that seen by Mr. Dawes, and might be due to the same kind of disturbance. In both instances the details of the appearances presented by the spots and faculæ are of an unusual character.

With reference to the second instance, I beg to state that on July 26, at 2^h P.M., I saw with the Northumberland Telescope of the Cambridge Observatory the remarkable spot which is the subject of Mr. Howlett's communication. Mr. Todd, the Junior Assistant, who called my attention to it in the temporary absence of Professor Adams, took a sketch of it. The sketch is, consequently, intermediate to those of July 25 and 27, figured in pp. 110 and 111 of the *Monthly Notices*, above cited. The peculiarity of small nuclei situated on the edges of large patches of penumbra is exhibited, with great changes in the details, in this as in the other sketches. What chiefly struck me was a bridge stretching across the principal nucleus, remarkably bright, and not bounded by a penumbra.

I judged it to be brighter than the general surface of the Sun. Mr. Howlett, describing its appearance on July 25, calls it "a remarkable mass of brilliant photospheric matter," at which time it had not decidedly assumed the form of a bridge. This phenomenon and the peculiarity above mentioned, with the dislocated appearance of the adjacent patches of penumbra, seem to be indicative of an unusual amount of disturbance.

With respect to indications of the existence of atmospheres to the planets, little can be said relative to the effects of atmospheric refraction. From what was proved at the beginning of this communication, the apparent semidiameters will most probably not be sensibly increased if the refraction be of the first kind; and if of the second kind, the increase would be the angle which the whole height of the atmosphere subtends at the Earth, and would vary with distance according to the same law as the apparent semidiameter. Supposing the refraction of *Jupiter's* atmosphere to be of the first kind, a satellite would probably be eclipsed as soon as the atmosphere interferes with its visual direction, because the deviation and divergence which the rays undergo by incidence on the atmosphere, might at once render them invisible at the great distance of the Earth. If otherwise, the satellite would diminish in brightness before disappearing, and such diminution might be taken as evidence of the existence of an atmosphere. If the refraction were of the second kind, the satellite would disappear without diminution of brightness, just as if it had been eclipsed by an opaque body. The most direct physical evidence of a planet's atmosphere would be the constant recurrence of variable appearances on the disk known not to be due to rotation, it being hardly conceivable that such appearances can have any other origin than the suspension and motion of cloud-like substances in an atmosphere. Such phenomena are observable on the disks of *Jupiter* and *Saturn*, and, as would seem from recent observations, on that of *Mars*.

It remains to notice one other phenomenon indicative of an atmosphere: a distant spherical body, whether self-luminous or shining by reflected light, would, according to the law that the intensity of the radiation varies as the sine of the angle which the direction of emanation makes with the surface, appear equally bright at all points of the disk. But an atmosphere to the body, of considerable magnitude and constituted like the Earth's, would intercept more of the rays coming from the parts near the periphery than of those coming from near the centre of the disk, because the former would pass through a greater thickness of atmosphere. There would, in consequence, be a gradual diminution of brightness from the centre to the circumference. This phenomenon is, in fact, plainly discernible in a photographic picture of the Sun, or in an image received on a screen, and is so simply explained by the above considerations, that it almost justifies of itself the inference that

the Sun has an atmosphere. The effect would be produced whether the atmospheric refraction be of the first of the two kinds before mentioned, or the second, but would be less conspicuous in the latter case than in the other. With respect to *Jupiter*, also, I have frequently remarked, that the features of the disk appeared more distinct near the centre than towards the periphery; and on the occasion of the occultation of this planet on January 2, 1857, I made the note that when it emerged from the Moon's bright limb, it appeared "brighter at the central parts than towards the border, and had in consequence something of a pellucid appearance." (See *Monthly Notices*, vol. xvii. p. 138). These phenomena are readily explained by the supposition of an atmosphere.

Cambridge, April 13, 1863.

On an Eclipse of the Sun recorded in the Chinese Annals as having occurred at a very early Period of their History. By Mr. Williams, Assistant-Secretary.

A few weeks since Mr. Hind requested me to look into the annals of China, with a view of ascertaining the particulars of a very early Solar Eclipse, said to be recorded in their most ancient historical work, the *Shoo King*, a copy of which is in my possession. I accordingly undertook the task, and the information thus obtained appearing to be of considerable interest, I am induced to lay it before the Society, it being the earliest record known of a Solar Eclipse. I therefore will very briefly give the result of this investigation, explaining, at the same time, the process by which it has been obtained. It is to be remembered that it is the Chinese account only I now lay before you, leaving its corroboration or disproval to those more competent than myself to enter into that part of the subject.

In the 書經 *Shoo King*—the most ancient historical work of the Chinese—in the second section of that work, under the 夏 Hea Dynasty, it is related that, in the reign of 仲康, Chung Kang, the fourth Emperor of that Dynasty, an Eclipse of the Sun took place. The passage recording this Eclipse occurs incidentally in an account of the delinquency of 羲 He and 和 Ho, two important officers, who appear to have had the superintendence of the Imperial Astronomical Board, as it may be called, and who, giving themselves up to wine, neglected their duties, and consequently, failing in the prediction of the Eclipse in question, rendered themselves

liable to punishment for that offence. The prediction of an eclipse was a matter of very great importance in China, as certain religious ceremonies were required to be performed, both by the Emperor and the people on these occasions, of which it was necessary that due notice should be given, in order that the requisite preparations should be made.

The passage recording the Eclipse is as follows, beginning to the right and reading downwards :—

集 Tseih	朔 So	季 Ke
于 Yu	辰 Shin	秋 Tsew
房 Fang	弗 Foo	月 Yue

which may be rendered, "In the last month of the autumn, the first day of the moon, the heavenly bodies were not in agreement in Fang."

I have not hitherto been able to find, in the *Shoo King*, any more definite date assigned to this Eclipse; but in other historical works in my possession the following passage occurs in each, which supplies this deficiency :—

元 Yuen	夏 Hea	壬 Jin
歲 Suy.	仲 Chung	戌 Seuh
	康 Kang	

This passage requires some explanation to render the observations that follow intelligible. The first two characters (Jin Seuh) are the cyclical characters of the year in question; the next (Hea) is the name of the dynasty; the next two (Chung Kang) are the name of the Emperor; and the last two (Yuen Suy) express "the first year." This sentence, then, is to be rendered, "In the year Jin Seuh of the Cycle, the first year of Chung Kang of the Hea Dynasty." The account that immediately follows this passage does not directly relate to the subject of the Eclipse, and is followed by the before-mentioned words of the *Shoo King*, commencing with "Tsew," and with the character for nine before the word "Yue" (month), consequently reading, "In the autumn, the ninth month, the first day of the moon," &c.

The notes in each of these works explain this passage as relating to an Eclipse of the Sun. In two of them we have a

further explanation of some of the expressions employed. Thus "Shin," the fifth character, is said to signify the "Sun and Moon;" and it is intimated that these luminaries are described as not being in "harmonious agreement in the division Fang."

It must, however, be mentioned, that in the great collection of Chinese historians, in three hundred volumes, entitled *She ke*, "*Historical Annals*," the early part of which was compiled in the century before the Christian era, although the neglect of He and Ho and their subsequent punishment are mentioned, no account of the occurrence of an Eclipse is given.

We have then, in the passage just quoted, a positive date assigned to this Eclipse, viz. "Jin Seuh," a certain year of a cycle, and the ninth month of that year. It remains, then, to show how this date is to be expressed in our notation. The mode of effecting this is, when explained, exceedingly simple, and the following is the course to be pursued:—The Chinese regulate their chronology by periods or cycles of 60 years. The years of these cycles are indicated, not by numerals, but by certain characters called by them Kea Tsze. These they divide into two series, the one of ten, the other of twelve characters, and the name Kea Tsze is derived from the names of the first characters in these series. They are employed in the following manner:—Combining the first characters in each series, we obtain the appellation of the first year of the Cycle, Kea Tsze. The second characters are combined for the second year, and so on to the eleventh, when the first character of the shorter series combines with the eleventh of the longer; and at the thirteenth the third of the shorter series combines with the first of the longer for the appellation of that year. Thus it goes on until, in the sixtieth year, the two last characters combine to form 60, and are followed in the sixty-first year by Kea Tsze, as before, when a new cycle commences.

We are now prepared to investigate the date of this Eclipse, as given in the Chinese annals. Upon examination, the characters at the commencement of the passage quoted (Jin Seuh) are found to be those answering to the fifty-ninth year of the cycle; and this particular cycle is considered by the Chinese to be the eighth of their series. Now it is reckoned that the seventy-fifth cycle commenced in the year 1804 of our era: consequently, seventy-four cycles were completed in 1803. Hence $74 \times 60 = 4440$ will give the number of years elapsed since the commencement of this mode of reckoning to 1803. Subtracting from that number the years of the Christian era, 1803, we obtain 2637 B.C. for the first year of the first cycle. Now we gather from the text, that the first year of Chung Kang was the fifty-ninth year of the eighth cycle, and, consequently, that seven cycles and fifty-nine years would be completed in that year. Consequently, that year was $7 \times 60 + 59 = 479$ years from the commencement of the cyclical mode of reckoning. Subtracting this from 2637,

we obtain 2158 B.C. as the date of the accession of Chung Kang; and as the eclipse occurred in the autumn of the same year, it must also be the date of that phenomenon.

We have now to consider the part of the heavens mentioned as the place where the eclipse took place; and here I may be allowed a few words in explanation of the mode in which the Chinese astronomers arrange the stars.

The visible heavens are divided by them into thirty-one parts. Three of these, called "yuen," are of considerable size. The first, called "Tsze wei yuen," comprises the Circumpolar stars, and may be considered as including those that never set in China. The second, named "Tai wei yuen," is bounded to the south by stars in *Virgo* and *Leo*, and includes to the north *Coma Berenices*, *Canes Venatici*, and some stars in *Ursa Major*. The third, "Teen she yuen," is bounded by stars in *Serpens* and *Hercules:* thus including the greater portion of *Ophiuchus*, the constellation *Hercules*, and other stars to the north.

The remaining twenty-eight divisions, called "Suh," of which *Fang* is one, may be considered as representing the lunar houses. They, however, are very irregular in extent, both from east to west and from north to south. Thus, while the smallest is less than one degree from east to west, the largest extends thirty-one degrees, and of the rest no two are alike in dimensions. In like manner, while one extends north and south from *Perseus* to *Canopus*, another consists merely of a few small stars in the head of *Orion*. Each of these twenty-eight divisions is determined by a certain group of stars having the name of the division, but frequently forming a very small portion of it, the name being common to the whole division, which may include various other asterisms wholly unconnected with the group thus named.

Thus, in the most extensive division Tsing, the group of stars so called consists of γ, ι, λ, μ, and other stars in *Gemini*, but the whole division extends 31° from east to west, and from *Perseus* in the north to *Canopus* in the south. It is necessary, in speculating on any phenomenon recorded in the Chinese books, to keep these circumstances in view, as an appearance described in these records as having occurred in Tsing, may be referred to any part of that division, and thus, without more definite information, its probable place cannot be identified with any degree of certainty.

The division Fang, in which this Eclipse is said to have occurred, is a very small one, being only about five degrees from east to west, and of but little extent from north to south. Its determining group consists of the stars β, δ, π, and ρ in *Scorpio*. It includes also a few small stars in *Libra* and *Ophiuchus* to the north, and in *Lupus* to the south. This, then, is the part of the heavens in which the Eclipse occurred; and, as it is a Solar Eclipse that is recorded, I presume there would be but little

difficulty in ascertaining the exact place of the Sun at the time mentioned.

The result then of this investigation, as far as I have carried it, is, that a Solar Eclipse is recorded in the most ancient Chinese historical work extant, as having occurred in the reign of Chung Kang, of the Hea dynasty, which Eclipse is more circumstantially mentioned by subsequent historians as having taken place in the first year of that Emperor, being the fifty-ninth year of the eighth cycle. That according to their mode of reckoning, the date of this Eclipse, expressed in our numbers, is 2158 B.C. and that it occurred in the ninth month of that year, in their division Fang, that asterism being part of our constellation *Scorpio*.

I have thus given the account of this Eclipse, simply as it occurs in Chinese historical works of reputation. There are, however, discrepancies in the year of the founding of the Hea dynasty, both in Chinese and European authorities, which may require attention. Leaving out one Chinese author, whose difference is upwards of 200 years, I find the first year of Yu, the founder of that dynasty, was according to one authority 2206, and according to the historians I have followed in this investigation 2204 B.C. According to what may be called European authorities, we have in Du Halde 2208, and in Gaubil 2205, as the date of that event. We have here then a difference of about four years, within which limits, I presume, should a Solar Eclipse be found to have occurred, satisfying the conditions as to time and place, it may reasonably be considered as that recorded in the curious and ancient book from which the preceding account has been taken.

I have also thought it necessary to give important passages in the original characters, knowing from experience the extreme difficulty of ascertaining with any degree of certainty the actual words intended to be employed, when their equivalent sounds are expressed in European letters only.* Whether a Solar Eclipse, visible in China, actually occurred at or near the time mentioned must be left for others to determine; and I can only say in conclusion, that should such be found to have been the case, it will afford a strong evidence of the general veracity of the early annals of that very ancient and interesting people.

On the Value of the Solar Parallax deduced from the Lunar Tables. By P. A. Hansen.

In the *Monthly Notices*, vol. xxiii. No. 7, Mr. E. J. Stone discusses different values of the co-efficient of the Parallactic

* For the loan of the Chinese type employed in this paper, the Society is indebted to Mr. Watts, Printer, of Crown Court, Temple Bar.

Equation of the Moon, and closes his note with the following words:—"It would be a point of interest to determine whether Professor Hansen's theory would bear any considerable increase in the mean Solar Parallax." I fulfil this desire with all the more pleasure, since the determination of the Solar Parallax from my Lunar Tables may be obtained by means of data which have been already published.

On page 177 of the *Darlegung der theoretischen Berechnung der in den Mondtafeln angewandten Störungen*, it will be found, that in my calculation of perturbations for the ratio of the major semi-axis of the orbits of the Earth and Moon, I have adopted the following value:—

$$\log \frac{a}{a'} = 74034757$$

which gives, for the co-efficient of the Parallactic Equation $121''\cdot368$ (Lunar Tables, p. 8). By comparison with observations, however, it results that all inequalities, theoretically calculated with the above value of $\frac{a}{a'}$, must be multiplied by the factor 1·03573 in order to conform to the observations. When, therefore, the above-mentioned co-efficient is multiplied by this factor, it gives $125''\cdot70$, which coincides very nearly with the value which Mr. Airy has obtained by the Altazimuth.

Further, in the Lunar Tables (p. 4), the constant of the Parallax of the Moon is given as $= 56' 59''\cdot57$. This being multiplied by the above value $\frac{a}{a'}$, we have $8''\cdot66$ for the Solar parallax, which has been adopted as the basis of the theoretical calculations. This, again, being multiplied by the above factor 1·03573, the Solar Parallax, determined by the elaboration of the Lunar Tables, becomes $8''\cdot97$. This value coincides very nearly with that which has been determined by the last opposition of *Mars*.

On the Companions of Sirius. By M. Goldschmidt.

(*Extract from a Letter to Admiral Manners, Secretary of the Royal Astronomical Society.*)

I have seen with satisfaction the insertion of my letter from M. l'Abbé Moigno's journal, *Les Mondes*, in your *Monthly Notices*, No. 5, vol. xxiii. I hope that *Sirius* will be observed next autumn by astronomers provided with large telescopes, to verify the minute stars I saw in its following vicinity. As long as *Sirius* was high enough above the horizon, I could see them; but afterwards I perceived only mere glimpses of light,

without being able to distinguish the position of the stars. On the top of the wood-cut in the *Monthly Notices*, No. 5, is written "*Position des Satellites:*" these words are not mine, for I cannot believe that they are all satellites. In the possibility that some of these luminous points could perhaps be the result of optical illusion or sickness of my eyes, I tried the same kind of observation (by passages) with some brilliant stars, like *Altair* and *Antares*, but I could see nothing like that I perceived on *Sirius;* and so, having done all that is in my power, with an object-glass of only 4 inches (English measure) or 46 lines, to snatch such minute light points, I leave it to more skilful astronomers to do the rest; for a powerful telescope with clock-work and the star hidden behind a large plate would show this wondrous system. The distance of the star D I tried to measure on the moment of its discovery, and it might be distant east from *Sirius* $3^s{\cdot}60$ or $3^s{\cdot}80$ in Right Ascension. I found also another fainter and distant star (perhaps 10″) in the vicinity, and on the east of the star D, which I announced in my first letter to Mr. Peters, on the 8th of March. On writing this, I received the news of Rev. Mr. Dawes' observation of the star D. It is, indeed, the most conspicuous of all by its *distant position*; but I think that the two nearest ones are more brilliant, and seem to be bodies of great importance relative to *Sirius*.

Paris, le 5 *Juin*, 1863.

On the Discordance between the Results for Zenith Distances obtained by direct Observation and those obtained by Observation by Reflexion from the Surface of Quicksilver. By G. B. Airy, Esq., Astronomer Royal. (Abstract.)

After explaining the manner in which the discordance in question is practically taken account of at the Greenwich Observatory, the author remarks that, in preparing for the formation of the new Seven-Year Greenwich Catalogue of Stars, it appeared to him desirable to examine carefully the results of the reductions (extending over a quarter of a century) which have been conducted on the principle described, with the view of ascertaining whether the Direct Observations ought to be used alone, or whether the Reflexion Observations ought to be used alone, or whether the mean of them ought to be used, as has been done in the Annual Reductions; and that the object of the Memoir is to give the result of this inquiry. It appears that neither the D observations nor the R observations can be adopted, without correction, as sufficiently accurate, but that the D observations corrected by $\frac{1}{2}$ (R — D), or still better by $\frac{3}{5}$ (R — D), give accordant values

of co-latitude, and may be received as perfectly accurate; and it is inferred that the correction $\frac{1}{2}$ (R — D) or $\frac{3}{5}$ (R — D) is a real correction, founded on some physical cause. There is no room for explanation from defects of the instruments. A suggestion is, however, offered as to the nature of the physical cause of R — D, viz. from a comparison of the results for three different periods, for which different shutter-openings were used in the observing-room, it appears to be probable that the origin of the discordance expressed by R — D lies in some conformation of the warmer and cooler strata of the atmosphere in the immediate neighbourhood of the Instrument. The concluding paragraphs of the Memoir are as follows:—

"It is to me a matter of great satisfaction to be assured that the enormous number of observations made at Greenwich from 1836 have been reduced on principles which are fundamentally correct, and which scarcely leave appreciable error. But I think that the investigation leads to inferences of a broader and still more important kind. It appears probable that no co-latitude of an Observatory, where direct observations alone are used, is certain to a quarter of a second, and no north polar distance of stars at 70° or 80° from the pole is certain to half a second. I know no method of removing this doubt but the introduction of reflexion observations of stars on both sides of the zenith.

"I ought not to close this paper without acknowledging that my first ideas of connecting the discordance of direct and reflexion observations with the circumstances of the observing room, were derived from a conversation with Mr. Faye, when I had the pleasure of seeing him at Greenwich, on the occasion of determining the difference of longitude between Paris and Greenwich in 1854. At the time of reading his valuable paper in the *Comptes Rendus*, 1850, September 16, there was no critical evidence as to the cause of the discordance; and even in 1854, when the observations as far as 1852 were thoroughly reduced, there was not a sufficient amount of evidence under the new circumstances of the Instrument to enable me to form a decided opinion. It is only with the accumulation of evidence in later years, exhibiting results so consistent among themselves, but so distinctly opposed to those of former years, that the possibility has presented itself of pronouncing with certainty on the general origin of the discordance, and on the way in which, astronomically, it ought to be treated."

Observations on the Planet Mars. By J. N. Lockyer, Esq. (Abstract.*)

The author mentions that during the opposition of last year, in spite of the generally very unfavourable state of the weather, he was able to make a series of sketches which embraced a complete rotation of the planet; and that the opposition of last year, occurring on the 5th October in heliocentric longitude 12°, afforded the opportunity, among other things, of fairly comparing the series with the one taken by MM. Beer and Mädler in 1830, when the opposition took place the 19th September, in heliocentric longitude 356°. As regards the permanency of the features, the general result is that the two series of drawings agree in a marvellous manner, the author's very much larger instrument revealing no main feature which was not foreshadowed in the earlier series, although, from the greater light of his own telescope, he was enabled to study the details in a more satisfactory manner. But, although the complete fixity of the main features of the planet seems to be thus ascertained beyond all doubt, there occur daily, nay hourly, changes of detail, and in the tones of the different parts of the planet, both light and dark—changes which, he doubts not, are caused by the transit of clouds over the different features.

The larger part of the Memoir consists of comments on the drawings, and the comparison of them with the earlier series.

Results of the Meridional Observations of Small Planets; Occultation of a Star by the Moon; and Phenomena of Jupiter's Satellites; observed at the Royal Observatory, Greenwich, in the months of April and May, 1863.

(*Communicated by the Astronomer Royal.*)

Astræa (5).

Mean Solar Time of Observation.	h m s	R.A. from Observation. h m s	N.P.D. from Observation. ° ′ ″
1863, April 1	9 9 5·8	9 47 59·70	73 14 55·73
2	9 5 17·8	9 48 7·59	73 14 3·28
7	8 46 45·0	9 49 14·56	73 12 21·02
18	8 8 28·3	9 54 13·62	73 25 28·52

* The Memoir, with the accompanying drawings, or a selection from them, will probably appear in the volume of *Memoirs*. The drawings were exhibited at the Meeting of the Society.—Ed.

Metis (9).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, May 13	13 20 27·4	16 45 37·84	111 51 40·16
21	12 41 16·0	16 37 52·48	111 52 27·18
22	12 36 18·6	16 36 50·80	111 52 21·44
23	12 31 20·8	16 35 48·75	111 52 14·02
25	12 21 23·4	16 33 42·78	111 51 57·23

Irene (14).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, April 7	9 21 44·7	10 24 19·96	64 43 7·77
13	8 58 11·5	10 24 22·20	65 12 29·61

Eunomia (15).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, April 7	12 25 40·0	13 28 45·53	117 34 4·92
20	11 22 43·5	13 16 53·86	116 35 0·70
23	11 8 17·2	13 14 14·79	116 17 42·81
30	10 34 58·1	13 8 26·15	115 33 51·84
May 4	10 16 14·2	13 5 25·34	115 7 10·54
7	10 2 20·5	13 3 19·10	114 46 44·39

Psyche (16).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, April 7	13 44 35·1	14 47 53·57	102 7 0·74
16	13 3 16·5	14 41 57·17	101 31 40·09
20	12 44 37·5	14 39 1·28	101 15 4·27
23	12 30 33·3	14 36 44·41	101 2 26·86
25	12 21 8·9	14 35 11·62	100 54 2·75
28	12 7 0·6	14 32 50·66	100 41 25·34
30	11 57 34·8	14 31 16·43	100 33 7·65
May 1	11 52 51·6	14 30 28·94	100 28 58·33
2	11 48 8·8	14 29 41·99	100 24 50·95
4	11 38 42·9	14 28 7·64	100 16 41·95
13	10 56 31·3	14 21 18·12	99 42 29·34
21	10 19 36·5	14 15 49·66	99 16 41·99
25	10 1 28·0	14 13 24·41	99 5 56·30
26	9 56 58·4	14 12 50·62	99 3 30·92

Bellona (36).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, April 1	8 23 18·6	9 2 5·03	71 1 39·41

Harmonia (40).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, April 16	13 7 16·2	14 45 57·53	99 31 24·01
20	12 47 53·2	14 42 17·56	99 15 7·71
23	12 33 12·5	14 39 24·06	99 3 3·94
25	12 23 22·5	14 37 25·55	98 55 3·58
28	12 8 34·4	14 34 24·74	98 43 20·96
30	11 58 41·6	14 32 23·43	98 35 47·96
May 1	11 53 45·0	14 31 22·56	98 32 7·53
2	11 48 48·8	14 30 22·12	98 28 28·57
4	11 38 56·1	14 28 20·83	98 21 26·91
7	11 24 10·0	14 25 22·00	98 11 32·79
8	11 19 15·4	14 24 23·15	98 8 27·66
13	10 54 52·8	14 19 39·37	97 54 41·76
21	10 16 43·4	14 12 56·12	97 39 28·71
22	10 12 2·8	14 12 11·31	97 38 13·24
23	10 7 23·6	14 11 27·86	97 37 12·66
25	9 58 9·7	14 10 5·63	97 35 27·66
26	9 53 35·2	14 9 26·94	97 34 54·25
28	9 44 30·6	14 8 13·95	97 34 9·68

Diana (78).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, April 15	10 0 8·2	11 34 22·23	96 10 13·04

All the observations of N.P.D. have been corrected for Refraction and Parallax.

Occultation of a Star by the Moon.

Day of Observation.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
1863.			h m s	
Apr. 26	κ Cancri, disapp.	Dark	9 44 24·0	C.
26	κ Cancri, reapp.	Bright	10 53 15·5	C.

Phenomena of Jupiter's Satellites.

Day of Observation. 1863.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
April 1	II	Eclipse, disapp.	12 18 35·7	E.
20	III	Ingress, bisection	14 14 42·7	E.
26	II	Eclipse, reapp.	11 51 25·5	C.
28	I	,, reapp.	11 6 45·3	E.
May 1	III	,, reapp.	11 26 4·9	R.
8	III	Occ. disapp. last cont.	10 39 53·4	N.
,,	III	Occ. reapp. last cont.	13 1 37·6	R.
,,	III	Eclipse, disapp.	13 3 52·3	R.
21	I	,, reapp.	11 17 34·9	E.
26	III	Egress, last cont.	9 30 59·6	C.
28	II	Eclipse, reapp. (*a*)	11 37 10·5	J. C.

(*a*) Probably late, the planet being very faint from cloud.

The initials E., C., J. C., R., and N., are respectively those of Mr. Ellis, Mr. Criswick, Mr. Carpenter, Mr. Roberts, and Mr. Nash.

Fig. 1.

Plato Sunrise. Rev. W. R. Dawes, 1863, April 26^d 1^h ±. Degree of illumination 0·03.

Fig. 2.

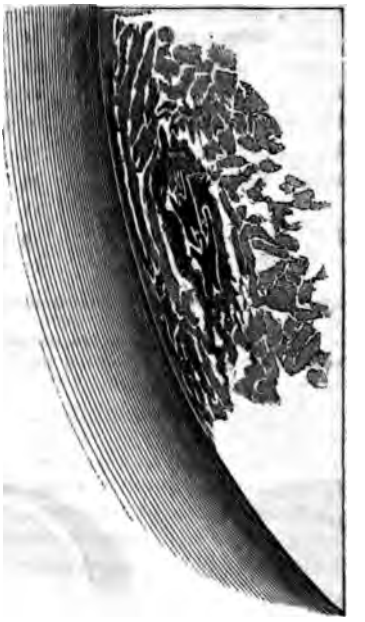
Plato Sunset. W. R. Birt, 1863, May 10^d 15^h 56^m. Degree of illumination 0·99.

Fig. 1 is intended to illustrate Mr. Dawes' paper on *Plato*, inserted in the last number of the *Monthly Notices*. The four crater spots on the floor are shown in the engraving. Fig. 2 exhibits the appearance of *Plato* at sunset, or more accurately within three hours of the sun setting in the western edge of *Plato*. Both sketches were taken during the same lunation, April and May, 1863.

Observation of a Solar Facula. By Capt. W. Noble.

While examining the Sun on the 24th of May, at 3^h, G.M.T., my attention was directed to a spot which was

just coming on to his following limb. Upon scrutinising the neighbourhood of this spot, I was at once attracted by the remarkable appearance presented by a facula situated between the spot referred to and the limb itself; an appearance depicted in the accompanying sketch. It was that



Part of the Sun's Limb, Sunday, May 24th, 1863. 3^h, G.M.T.

of a most extraordinary elevation above the general level of the Sun's apparent surface, the facula exhibiting quite a dark line on its following edge, and presenting a perfectly stereoscopic effect. My first impression was that this was an optical illusion, and that what I took for the shading on the following side of this enormous ridge of light was really a spot or macula excessively foreshortened. Upon looking at the Sun, however, when this suppositious spot *should* have arrived fairly on to his disk by his rotation, I found the neighbourhood a perfect blank, the spot, following which I had first observed the facula, being quite isolated. The drawing which I send was made, *ad naturam*, with a power of 154. Owing to the employment of a "Hodgson" eye-piece, a double reversal of the image has necessarily taken place. I have marked the facula *a*. The extreme length of the penumbra of the spot to the right of it, was as nearly as possible 0′·8 of arc, which will supply the scale to which the sketch was made.

Forest Lodge, Maresfield, Sussex,
June, 1863.

Eclipse of the Sun, May 17th, 1863.
By Capt. W. Noble.

The first indentation of the Moon upon the Sun's limb was observed about $9^h\ 24^m\ 37^s$ local sidereal time $= 5^h\ 44^m\ 55^s{\cdot}3$ local mean time. The Moon's limb was relatively smooth. The sky was very cloudy, and it was only at intervals that a distinct glimpse of the solar disk could be obtained. A slight break enabled the greatest phase to be observed. The Sun set densely obscured in a bank of clouds. Powers 42 and 74, approximate latitude and longitude $51^\circ\ 0'\ 58''$ N., and $17^s{\cdot}5$ E.

Forest Lodge, Maresfield,
11th June, 1863.

Total Eclipse of the Moon, June 1st, 1863.
By Capt. W. Noble.

The singularly undefined character of the edge of the Earth's shadow, as viewed in the telescope, rendered anything like observations of Time hopeless. At the time of the middle of the eclipse, the lunar detail was well seen with a power of 42, save just in the centre of the shadow, where everything was obscured. I cannot remember ever to have seen such a nebulous density (if I may coin that expression) in any of the lunar eclipses which I have hitherto observed.

Forest Lodge, Maresfield,
11th June, 1863.

Occultation of κ Cancri by the Moon, 26th April, 1863.
By Capt. W. Noble.

The star disappeared instantaneously at $12^h\ 3^m\ 15^s{\cdot}1$ local sidereal time $= 9^h\ 45^m\ 41^s{\cdot}42$ local mean time. Power 115 adjusted on the star.

In the May number of the *Monthly Notices* is a paper by the Rev. W. R. Dawes on a very remarkable phenomenon observed by Mr. Copeland, near Manchester, upon the occasion of this occultation, viz., the extinction of three-fourths of the star's light just prior to its disappearance. I can only say that, as seen here, κ *Cancri* retained its intrinsic brightness up to the very instant of the Moon's covering it. I observed

with my Ross Equatoreal of 4·2 inches aperture, and although Mr. Copeland had the advantage of 0·8 inch in the diameter of his object-glass, my own instrument is one of such great and proved excellence, that I can only in his case, with great deference, suspect some optical illusion.

Observatory, Forest Lodge, Maresfield,
11th June, 1863.

Occultation of κ Cancri, 26th April, 1863. By J. Vertu, Esq.

I did not think it worth communicating to the Society the observation I made of the occultation of κ *Cancri* on the 26th of April last; but the communications of Messrs. Burr and Dawes, in the *Notices* for May, induce me to send this note.

Some time before the occultation, having ascertained the position of the star, I adjusted the focus to the Moon, and was not a little surprised to find the *lumière cendrée* so strong that the complete circle was sharply defined, and some features in the *dark* part discernible; so much so that I was only a few seconds wrong in the time of occultation by estimation. Now as such is, I think, seldom if ever the case in this country at that age of the Moon (8·8 days), and notwithstanding the isolated and exceptional cases mentioned by Gruithuisen and Kunowsky, it would be desirable to ascertain the cause of such an anomaly—whether it be produced by an exceptional condition of the atmosphere, by the Sun's light being reflected on our satellite by some particular spot of the Earth's surface, or otherwise. It cannot be attributed to the instrument; for, with the same telescope and eye-piece, I watched the new Moon again, but after the sixth day I could perceive no trace of the Earth's light.

As regards the occultation, which I watched with increased attention, the star, which I followed until in contact with the Moon's edge, disappeared *instantaneously*. It seems to me highly improbable that a star—even supposing it to be double—which under a power of 1000 has given a round disk, would produce a perceptible diminution of light: I should rather attribute such an impression to the excitement produced on the retina by an intense and protracted gaze at the object.

It is true that "*Visus est acerrimus sensuum;*" but, *à fortiori*, this adage holds good in scrutinising the star in the telescope.

The telescope used in this observation is a $3\frac{1}{2}$-inch, by Troughton and Sims; power, 84.

Nottingham, June 11, 1863.

Observations of Astræa (5), *and of Solar Eclipse, May* 17, 1863. By J. Hartnup, Esq.

Observations of *Astræa* (5), taken with the Equatoreal of the Liverpool Observatory:—

1863.	G.M.T. h m s	R.A. (5) h m s	N.P.D. (5) ° ′ ″	Star of Comparison.
March 3	8 50 43·0	9 57 16·96	75 14 49·7	Regulus.
,, 4	9 31 58·5	9 56 35·06	75 7 44·3	,,
,, ,,	9 41 56·2	9 56 34·79	75 7 41·3	,,
,, ,,	9 51 54·6	9 56 34·39	75 7 36·9	,,

The observations are corrected for refraction and parallax. The place of *Regulus* was taken from the *Nautical Almanac*.

End of Solar Eclipse, observed with the Equatoreal of the Liverpool Observatory:—

1863, May 17 $7^h\ 10^m\ 48^s{\cdot}77$ G.M.T.

End of Solar Eclipse, observed by John Joynson, Esq., at Waterloo, near Liverpool:—

Lat. 53° 28′ 24″ N.; long. 3° 1′ 44″ W.

1863, May 17 $7^h\ 10^m\ 46^s{\cdot}5$ G.M.T.

Observed with a Refractor of 3½-inches aperture and 50-inches focal length. The time was taken from a clock which is controlled by the Normal Clock at the Liverpool Observatory.

May 17, 1863.

Observation of Solar Eclipse, 1863, *May* 17. By Lord Wrottesley.

The end of the Solar Eclipse of the 17th of May was observed at my Observatory at Wrottesley, by my Assistant Mr. Hough, to take place at $7^h\ 2^m\ 17^s$, Wrottesley Mean Time. Telescope, the 11-foot Equatoreal; aperture, 7¼ inches.

The same phenomenon was observed by myself, with my 45-inch Achromatic and Baird Pocket Chronometer,* No. 2962, at $7^h\ 2^m\ 10^s$, Wrottesley Mean Time.

The error of the Transit-clock was obtained by the observation of the Transit of a Greenwich Star during the Eclipse.

* This is the instrument which I lent to Sir Leopold M'Clintock, and which accompanied him during his last Polar Expedition.

The chronometer was compared immediately after the termination.

The beginning was lost through clouds. It became clear a short time after the commencement, and then the cusps and the limb of the Moon were sharply defined, and afterwards the limb became very tremulous; it was also very much jagged, as viewed in the larger telescope, resembling the teeth of a saw.

Observations of Occultations of Stars by the Moon; of Eclipses of the Sun and the Moon; and of Eclipses of Jupiter's Satellites; made at the Radcliffe Observatory, Oxford. Communicated by the Rev. R. Main.

Occultations of Stars by the Moon.

Day. 1863.	Star.	Phenomenon.	Observer.	Instrument.	Oxford M.T. h m s
Jan. 27	ϑ Arietis	Disap.	M.	Heliometer	5 3 52·7
	,,	,,	Q.	10-ft. telescope	5 3 53·5
	ϑ Arietis	Reap.	M.	Heliometer	6 21 6·2
Mar. 2	α Cancri	Disap.	L.	10-ft. telescope	11 42 12·6
April 29	e Leonis	Disap.	L.	,,	10 20 41·6
	e Leonis	Reap.	L.	,,	11 21 43·2

The initials M., Q., and L., are those of Mr. Main, Mr. Quirling, and Mr. Lucas.

Eclipse of the Sun, 1863, *May* 17.

Clouds prevented the beginning of the eclipse from being accurately observed; the time noted when they had cleared away being several seconds too late.

The end was, however, well seen by Mr. Lucas with the 46-inch achromatic telescope mounted on the leads of the Observatory outside the town.

The observed Oxford Mean Solar Time of the last contact was $7^h\ 6^m\ 32^s{\cdot}4$.

Eclipse of the Moon, 1863, *June* 1.

Mr. Lucas observed the beginning and ending of this eclipse with the 10-feet telescope, near the Heliometer Dome.

The Oxford Mean Solar Times are as follow:—

		h m s
First contact with the shadow	...	9 41 36
Beginning of total phase	...	10 46 40
Last contact with the shadow	...	13 0 57

Eclipses of Jupiter's Satellites.

Observed by Mr. Main with the telescope of the Heliometer: —

1868.		h m s	
April 1	Disappearance of 2d Satellite at	12 14 18·5	Oxford M.T.
28	Reappearance of 1st Satellite at	11 1 41·0	"

M. Foucault's Apparatus for producing Uniform Motion.

(*Communicated by Lieut.-Col. R. Strange.*)

On a recent visit to Paris, M. Foucault was good enough to show me the new clock-work, just completed by the firm of Secretan, intended to drive the Great Equatoreal of the Paris Observatory.

M. Foucault has for some time been occupied with this subject, and has at length arrived at results with which he is himself perfectly satisfied, and which are also considered equally satisfactory by the authorities at the Observatory—the motion controlled by this apparatus being sensibly uniform under all possible changes in the motive force, and variations of friction and resistance.

I myself witnessed the following experiment:—The clock-work being detached from the Equatoreal (to which it had not yet been finally fitted), the full weight (about 58 lbs.) was applied; and the instrument being adjusted for rate, the revolving index was timed by a chronometer for about half an hour, during which period no sensible variation of rate of revolution could be detected. Half the driving weight was then removed without stopping or in any way readjusting the instrument, and the revolution of the index was again carefully observed, but no variation of the smallest perceptible fraction of a second could be detected.

M. Foucault assured me that the instrument had been subjected to far more severe and protracted trials than the above, and with the same results; and he affirms that, for all practical purposes, the problem of obtaining uniformity of rotation under considerable changes of resistance is absolutely solved.

I shall not attempt to describe from memory a machine of which a full account will shortly be published by its distinguished contriver, by whom this important invention has been patented in France and England. My object in mentioning the subject is to induce those who are either engaged in the

same research, or who propose ordering any contrivance of this kind, to pause until M. Foucault's arrangement is made known.

I should add, that I have M. Foucault's permission to make the above communication to the Royal Astronomical Society.

London, June, 1863.

Ephemeris of Winnecke's Comet of Short Period, about the Time of Perihelion Passage, 1863. By J. R. Hind, Esq.

(*From Dr. Seeling's Elements, Ast. Nach. No.* 1319.)

Greenwich Mean Midnight.	R.A.		Decl.		Log. Dist. from Earth.		Log. Dist. from Sun.	
1863.	h m	m	° ′	° ′				
Nov. 2	15 9·1		−13 23		0·2569		9·9265	
		+19·7		−1 42		−77		−131
6	15 28·8		15 5		0·2492		9·9134	
		20·7		1 38		72		111
10	15 49·5		16 43		0·2420		9·9023	
		21·6		1 34		66		85
14	16 11·1		18 17		0·2354		9·8938	
		22·5		1 27		58		56
18	16 33·6		19 44		0·2296		9·8882	
		23·3		1 18		49		−22
22	16 56·9		21 2		0·2247		9·8860	
		23·8		1 7		39		+11
26	17 20·7		22 9		0·2208		9·8871	
		24·3		0 55		28		45
30	17 45·0		23 4		0·2180		9·8916	
		24·7		0 42		17		75
Dec. 4	18 9·7		23 46		0·2163		9·8991	
		24·8		0 26		−4		103
8	18 34·5		24 12		0·2159		9·9094	
		24·7		−0 12		+10		125
12	18 59·2		24 24		0·2169		9·9219	
		24·3		+0 3		23		142
16	19 23·5		24 21		0·2192		9·9361	
		23·8		0 17		37		155
20	19 47·3		24 4		0·2229		9·9516	
		23·1		0 30		49		164
24	20 10·4		23 34		0·2278		9·9680	
		22·2		0 41		61		170
28	20 32·6		22 53		0·2339		9·9850	
1864.		21·3		0 50		72		172
Jan. 1	20 53·9		22 3		0·2411		0·0022	
		20·4		0 59		81		172
5	21 14·3		21 4		0·2492		0·0194	
		19·3		1 5		90		170
9	21 33·6		19 59		0·2582		0·0364	
		18·3		1 10		97		168
13	21 51·9		18 49		0·2679		0·0532	
		17·4		1 13		102		164
17	22 9·3		17 36		0·2781		0·0696	
		16·5		1 16		107		160
21	22 25·8		16 20		0·2888		0·0856	
		15·6		1 16		110		156
25	22 41·4		15 4		0·2998		0·1012	
		14·8		1 17		111		151
29	22 56·2		13 47		0·3109		0·1163	
		14·1		1 16		113		146
Feb. 2	23 10·3		12 31		0·3222		0·1309	
		13·3		1 16		114		141
6	23 23·6		11 15		0·3336		0·1450	
		12·7		1 15		113		137
10	23 36·3		10 0		0·3449		0·1587	
		+12·2		+1 12		+112		+132
14	23 48·5		−8 48		0·3561		0·1719	

By Dr. Seeling's elements (neglecting perturbations, which, during the present revolution, are likely to be small) the Comet should arrive at perihelion on Nov. 23·178 Mean Time at Greenwich.

Nautical Almanac Office,
1863, *July* 1.

A note on the Eclipse of the Moon on June 1st, as seen at Exeter, accompanied by three drawings, which were exhibited at the Meeting, was received from Henry S. Ellis, Esq.

A note was also received from Mr. Thomas Petty, mentioning his observation on the morning of April 27 of a small but bright Comet, being in fact Comet III., 1863, see *Monthly Notice*, No. 7, pp. 226 and 227.

Corrections to the Translation of Prof. Wolf's Letter, Monthly Notices, May 8, 1863, *No.* 7.

(*Communicated by the Astronomer Royal.*)

Page 208.—The translation from the German is, perhaps a little too literal; and, to remove obscurity, the following modification is desirable:—

Lines 6 and 7, *inclose* in brackets ( ) the clause beginning with "not to enter," and ending with "in No. 13."

Lines 20, 21, and 22, *inclose* in brackets ( ) the clause beginning "when occupying myself," and ending "ϑ (*Argo*) Navis."

Page 228.—Line 20 from the bottom, *for* star, *read* object.

Instruments for Sale.

Astronomical Telescope and Sidereal Clock.—The Telescope is by Tully, 5 ft. 6 in. focal length, 4-in. field, with three common and four astronomical powers, from 60 to 270; rack, vertical and horizontal movements, with Vernier scale to each; mounts on a strong mahogany stand, on a patent equatoreal block; main tube screws in midships, and packs up in a mahogany case, with lamp and illuminating reflector. The Clock by Barraud. Both formerly the property of the celebrated Astronomer, Baily.—Apply to J. Stebbing, Esq., F.R.A.S., Southampton.

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MONTHLY NOTICES

OF THE

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*Mécanique Céleste: Observations sur la Comparaison établie par M. Delaunay entre les Expressions des Coordonnées de la Lune déduites de sa Théorie avec celles qui avaient été obtenues antérieurement.** Par M. G. de Pontécoulant.

Rien sans doute ne saurait être plus utile au perfectionnement de la théorie de la Lune que la comparaison des expressions, obtenues par des méthodes différentes, des trois coordonnées qui fixent à chaque instant la position de cet astre, et qui doivent servir à la construction des tables lunaires. Aussi M. Delaunay a fait, sous ce rapport, un travail très recommandable lorsqu'il a dernièrement comparé les résultats obtenus par M. Plana dans sa Théorie de la Lune, à ceux qu'il a déduits lui-même de la méthode qu'il a suivie pour arriver au même but; mais comme l'auteur n'a publié jusqu'à présent que l'exposition de la méthode, et que le second volume de son ouvrage, qui doit contenir les applications numériques, n'a pas encore paru, il est tout-à-fait impossible, au moins quant à présent, de découvrir les véritables causes qui ont produit les différences qu'on remarque entre quelques uns de ses résultats et ceux de M. Plana, et de décider d'une manière certaine de quel coté est l'erreur, puisque d'après le mode de comparaison adopté par M. Delaunay, ces résultats devraient être parfaitement identiques. Cependant il m'a paru d'après la seule inspection des tableaux de M. Delaunay que plusieurs des résultats qu'il présente peuvent être avec raison

* Voir les *Comptes Rendus* de l'Académie des Sciences; 1862, 1er Sémestre. (T. liv. Nos. 15, 16, 17, et 18.)

regardés comme entachés d'inexactitude; et c'est à provoquer sur ce point une utile discussion entre les géomètres qui se sont occupés de la théorie de la Lune, pour arriver enfin à des formules parfaitement correctes, telles qu'on doit les espérer de l'état actuel de la science, que cette note est spécialement destinée.

J'examinerai successivement les expressions des trois coordonnées de la Lune, *la longitude, la latitude, et la valeur inverse du rayon vecteur*, en suivant l'ordre adopté par M. Delaunay, en négligeant d'abord comme il l'a fait les termes d'un ordre supérieur au 7e: nous négligerons également pour abréger le travail, à moins de quelques circonstances particulières, les termes indépendants de la quantité très petite m, ou multipliés par une puissance de cette quantité inférieure à la 3e, parceque ces termes étant indépendants de la force perturbatrice, ou ne dépendant en général que de la première puissance de cette force, il sera toujours assez facile de les vérifier et de reconnaitre par conséquent la cause des anomalies qu'on remarquerait entre les résultats trouvés à cet égard par les différents auteurs. J'omettrai de même, pour abréger autant que possible cette note, les termes où les résultats de M. Delaunay, tout en s'éloignant de ceux de M. Plana, concordent avec ceux obtenus antérieurement par M. Lubbock, ou par moi, puisque les corrections qu'ils indiquent dans les valeurs données par M. Plana, peuvent être regardées désormais comme acquises à la science.

Dans les tableaux suivants je présenterai les résultats obtenus par M. Plana tels qu'ils sont donnés dans son ouvrage, et sans leur faire subir aucune transformation, et je ramenerai au contraire les résultats de M. Delaunay que je me proposerai de leur comparer, à ce qu'ils devraient être en adoptant pour l'excentricité e de l'orbe lunaire et son inclinaison γ à l'écliptique, les valeurs que M. Plana leur attribue; il m'a semblé que les différences des résultats en ressortiraient mieux, puisque ce sont en définitive les résultats de M. Plana qui doivent servir de terme de comparaison.

Dans une colonne particulière je présenterai en regard des résultats précédents, ceux que j'ai obtenus moi-même dans ma théorie des mouvements lunaires (*Théorie Analytique du Système du Monde*, t. iv.), et j'ajouterai au pied les observations succinctes qui peuvent résulter de cette double comparaison.

Les inégalités dont dépendent les différents termes que nous aurons à examiner, seront indiquées par leurs arguments dans lesquels :

D *représente la longitude moyenne de la Lune moins celle du Soleil.*

F *la longitude moyenne de la Lune moins celle du Nœud.*

l *l'anomalie moyenne de la Lune.*

l' *l'anomalie moyenne du Soleil.*

Longitude.

Arguments.	Termes de Plana.	Termes de Delaunay.	Théorie Analytique.
l'	$-\frac{39522017}{6144}\, m^4 e^2 e'$	$-\frac{66621857}{6144}\, m^4 e^2 e'$	$-\frac{39522017}{6144}\, m^4 e^2 e'$ (a) A*
	$+\frac{3257665}{576}\, m^6 e'$	$+\frac{3253129}{576}\, m^6 e'$	$+\frac{3257665}{576}\, m^6 e'$ (b) A*
$l-l'$	$+\frac{8548531}{6144}\, m^4 e e'$	$+\frac{2814537}{2048}\, m^4 e e'$	$+\frac{2844993}{2048}\, m^4 e e'$ (c)
	$+\frac{307373383}{36864}\, m^5 e e'$	$+\frac{103086725}{12288}\, m^5 e e'$	$+\frac{307187071}{36864}\, m^5 e e'$
$l+l'$	$-\frac{4173895}{6144}\, m^4 e e'$	$-\frac{2714815}{6144}\, m^4 e e'$	$-\frac{2806183}{6144}\, m^4 e e'$ (d)
	$-\frac{19962409}{9216}\, m^5 e e'$	$-\frac{42301831}{18432}\, m^5 e e'$	$-\frac{19962409}{9216}\, m^5 e e'$ (e)
$l-2l'$	$-\frac{489}{128}\, m e^3 e'^2$	$+\frac{153}{128}\, m e^3 e'^2$	$-\frac{1017}{256}\, m e^3 e'^2$ (f)
$2l$	$-\frac{6221}{192}\, m^2 e^4$	$-\frac{29543}{1536}\, m^2 e^4$	$-\frac{78109}{4608}\, m^2 e^4$
	$-\frac{567}{16}\, m^2 e^2 e'^2$	$-\frac{4089}{128}\, m^2 e^2 e'^2$	$-\frac{3809}{128}\, m^2 e^2 e'^2$
	$-\frac{5597}{64}\, m^4 e^2$	$-\frac{11319}{128}\, m^4 e^2$	$-\frac{5597}{64}\, m^4 e^2$ (g)
$2F-l$	$+\frac{21387}{2048}\, m^3 e \gamma^2$	$-\frac{1797}{2048}\, m^3 e \gamma^2$	$+\frac{2867}{2048}\, m^3 e \gamma^2$ (h)
$2D$	$-\frac{904829}{9216}\, m^3 e^4$	$-\frac{343199}{3072}\, m^3 e^4$	$-\frac{118199}{3072}\, m^3 e^4$
	$-\frac{761227}{6144}\, m^3 e^2 e'^2$	$-\frac{293497}{2048}\, m^3 e^2 e'^2$	$-\frac{264025}{2048}\, m^3 e^2 e'^2$
	$+\frac{144641579}{294912}\, m^5 e^2$	$+\frac{142735019}{294912}\, m^5 e^2$	$+\frac{143944619}{294912}\, m^5 e^2$
	$-\frac{70043}{768}\, m^5 e'^2$	$-\frac{61969}{864}\, m^5 e'^2$	$-\frac{91129}{864}\, m^5 e'^2$
$2D-l'$	$+\frac{7220939}{4096}\, m^4 e^2 e'$	$+\frac{7254731}{4096}\, m^4 e^2 e'$	$+\frac{7273931}{4096}\, m^4 e^2 e'$

* Les termes marqués d'un A ont été verifiées par M. Adams.

Arguments.	Termes de Plana.	Termes de Delaunay.	Théorie Analytique.
	$-\frac{245}{32}\, m e^4 e'$	$-\frac{105}{32}\, m e^4 e'$	$-\frac{35}{64}\, m e^4 e'$
$2D+l'$	$+\frac{14201441}{4096}\, m^4 e^2 e'$	$+\frac{44611649}{36864}\, m^4 e^2 e'$	$+\frac{4791425}{4096}\, m^4 e^2 e'$
	$+\frac{105}{32}\, m e^4 e'$	$+\frac{45}{32}\, m e^4 e'$	$+\frac{15}{64}\, m e^4 e'$
$2D+l$	$+\frac{940855}{82944}\, m^6 e$	$+\frac{1863229}{82944}\, m^6 e$	$+\frac{1228925}{41472}\, m^6 e$
	$-\frac{967447}{2304}\, m^4 e e'^2$	$-\frac{963415}{2304}\, m^4 e e'^2$	$-\frac{957907}{2304}\, m^4 e e'^2$
	$+\frac{2354603}{8192}\, m^4 e^3$	$+\frac{5433089}{24576}\, m^4 e^3$	$+\frac{6040589}{24576}\, m^4 e^3$
$2D-l$	$+\frac{3496767841}{2654208}\, m^6 e$	$+\frac{3514035745}{2654208}\, m^6 e$	$+\frac{3464046433}{2654208}\, m^6 e$ (i)
	$+\frac{127960397}{221184}\, m^5 e$	$+\frac{127571597}{221184}\, m^5 e$	$+\frac{127960397}{221184}\, m^5 e$ (j)
	$+\frac{36126953}{9216}\, m^4 e e'^2$	$+\frac{37031581}{9216}\, m^4 e e'^2$	$+\frac{37134073}{9216}\, m^4 e e'^2$
	$-\frac{326681}{1536}\, m^3 e e'^2$	$-\frac{322081}{1536}\, m^3 e e'^2$	$-\frac{326617}{1536}\, m^3 e e'^2$ (k)
	$-\frac{14428681}{24576}\, m^4 e^3$	$-\frac{8805001}{24576}\, m^4 e^3$	$-\frac{10219551}{24576}\, m^4 e^3$
	$-\frac{827639}{18432}\, m^3 e^3$	$-\frac{91071}{2048}\, m^3 e^3$	$-\frac{64221}{2048}\, m^3 e^3$
$2D-l-l'$	$+\frac{1211197}{8192}\, m^5 e e'$	$+\frac{1200185}{8192}\, m^5 e e'$	$+\frac{1202189}{8192}\, m^5 e e'$
$2D-l-2l'$	$+\frac{219639709}{73728}\, m^4 e e'^2$	$+\frac{218629693}{73728}\, m^4 e e'^2$	$+\frac{219825901}{73728}\, m^4 e e'^2$ (l)
$2D-l+2l'$	$-\frac{46669}{128}\, m^4 e e'^2$	$-\frac{46831}{128}\, m^4 e e'^2$	$-\frac{23417}{64}\, m^4 e e'^2$
$2D-2l$	$+\frac{2166605291}{884736}\, m^5 e^2$	$+\frac{1966421891}{884736}\, m^5 e^2$	$+\frac{2117652131}{884736}\, m^5 e^2$
$2D-l+l'$	$+\frac{2827212637}{221184}\, m^6 e e'$	$+\frac{2745937561}{221184}\, m^6 e e'$	$+\frac{2827212637}{221184}\, m^6 e e'$ (m)
$2D-2F$	$-\frac{168653}{18432}\, m^4 \gamma^2$	$-\frac{110333}{18432}\, m^4 \gamma^2$	$-\frac{110333}{18432}\, m^4 \gamma^2$ (n)

Arguments.	Termes de Plana.	Termes de Delaunay.	Théorie Analytique.
$4D - l + l'$	$-\frac{3547567}{8192} m^5 e e'$	$-\frac{241903}{8192} m^5 e e'$	$-\frac{363727}{8192} m^5 e e'$
D	$-\frac{1553407}{1024} m^5 \frac{a}{a'}$	$-\frac{2357119}{1536} m^5 \frac{a}{a'}$	$-\frac{4628333}{3072} m^5 \frac{a}{a'}$
$D + l'$	$-\frac{124805}{768} m^3 \frac{a}{a'} e'$	$-\frac{123365}{768} m^3 \frac{a}{a'} e'$	$-\frac{128485}{768} m^3 \frac{a}{a'} e'$
$D - l$	$-\frac{156147}{1024} m^3 \frac{a}{a'} e$	$-\frac{155267}{1024} m^3 \frac{a}{a'} e$	$-\frac{155267}{1024} m^3 \frac{a}{a'} e$ (o)
	$-\frac{14427729}{16384} m^4 \frac{a}{a'} e$	$-\frac{14382081}{16384} m^4 \frac{a}{a'} e$	$-\frac{14382081}{16384} m^4 \frac{a}{a'} e$

Observations.

(a) Notre valeur s'accorde avec celle de M. Plana, celle de M. Delaunay parait fautive.

(b) Idem.

(c) Notre valeur revue par M. Adams, ne laisse aucun doute sur son exactitude.

(d) Notre valeur employée dans le calcul du terme suivant qui s'accorde avec celui de M. Plana, est necessairement exacte.

(e) Notre valeur coincide avec celle de M. Plana.

(f) La valeur de M. Delaunay parait à la fois inexacte et de signe contraire à ce qu'elle devrait être.

(g) Il y a soupçon d'erreur dans les calculs de M. Delaunay puisque nous nous accordons avec M. Plana.

(h) On trouve le détail du calcul de ce terme assez difficile dans les *Comptes Rendus* de l'Acad. 1842.

(i) Comme ce terme dépend du terme en $m^5 e$ qui est inexact dans les calculs de M. Delaunay on ne saurait s'étonner de la différence qui existe entre nos valeurs.

(j) Nous nous accordons avec M. Plana.

(k) Notre terme employé dans le calcul d'autres quantités a donné des résultats exacts, ce qui le vérifie.

(l) Il entre dans le calcul de ce terme des quantités qui ne sont pas données par M. Plana, ce qui peut causer la différence qu'on remarque dans nos valeurs.

(m) L'accord de notre résultat avec celui de M. Plana parait prouver suffisamment que la valeur de M. Delaunay est fautive.

(n) La révision de nos calculs nous a amenés à constater l'exactitude de la valeur donnée par M. Delaunay.

(o) La révision de nos calculs nous a amenés à un accord parfait avec M. Delaunay.

*Latitude.**

Arguments.	Termes de Plana.	Termes de Delaunay.	Théorie Analytique.
$F - l$	$-\frac{45611}{2048} m^4 e \gamma$	$-\frac{46587}{2048} m^4 e \gamma$	$-\frac{45611}{2048} m^4 e \gamma$ (a)
$F + l$	$-\frac{2679}{128} m^4 e \gamma$	$-\frac{2739}{128} m^4 e \gamma$	$-\frac{2679}{128} m^4 e \gamma$ (a)
	$-\frac{1356205}{12288} m^5 e \gamma$	$-\frac{710431}{6144} m^5 e \gamma$	$-\frac{710431}{6144} m^5 e \gamma$ (b)
$2D - F$	$-\frac{4548169}{442368} m^5 \gamma$	$-\frac{4548169}{442368} m^5 \gamma$	$-\frac{4548169}{442368} m^2 \gamma$ (c)
$2D + F$	$+\frac{134555}{2048} m^3 e^2 \gamma$	$+\frac{126875}{2048} m^3 e^2 \gamma$	$+\frac{126875}{2048} m^3 e^2 \gamma$ (d)
$2D - F - l'$	$+\frac{25493}{512} m^4 e' \gamma$	$+\frac{158057}{4096} m^4 e' \gamma$	$+\frac{158953}{4096} m^4 e' \gamma$
$2D - F + l'$	$+\frac{24337}{4608} m^4 e' \gamma$	$+\frac{131363}{36864} m^4 e' \gamma$	$+\frac{134927}{36864} m^4 e' \gamma$
$2D - F - 2l$	$+\frac{435649}{6144} m^3 e^2 \gamma$	$+\frac{807091}{12288} m^3 e^2 \gamma$	$+\frac{808711}{12288} m^3 e^2 \gamma$ (e)
$4D - F$	$+\frac{127483}{122880} m^5 \gamma$	$+\frac{456643}{122880} m^5 \gamma$	$+\frac{456643}{122880} m^5 \gamma$ (f)

Observations.

(a) Nos valeurs s'accordent avec celles de M. Plana, les corrections proposées par M. Delaunay paraissent donc inéxactes.

(b) M. Delaunay adopte la rectification que nous avons fait subir au terme de M. Plana, quoiqu'il ait omis de le remarquer dans son tableau.

(c) Le terme donné par M. Plana coincide avec le mien en adoptant la formule de transformation corrigée par M. Adams. Son accord avec celui donné par M. Delaunay confirme son exactitude.

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* Il y a relativement à l'expression de la latitude une observation générale à faire; j'avais adopté dans ma théorie le coefficient du terme dependant de l'argument de la latitude, tel qu'il est donné par M. Plana. Mais M. Adams ayant rectifié une erreur parmi les quantités du 5e ordre dans l'expression de ce coefficient†. erreur qui influe sur tous les autres coefficients de l'expression de la latitude, il faut pour en tenir compte multiplier par le facteur $\left(1 + \frac{121}{256} m^4\right)$ tous les termes de M. Plana, avant de les comparer avec nos résultats et ceux de M. Delaunay. Cette simple correction fait disparaitre, comme on peut s'en assurer, une grande partie des différences qu'on remarquait entre nos valeurs et celles de M. Plana, et les réduit relativement à l'expression de la latitude à un très petit nombre de termes.

† *Comptes Rendus*, 1862, 1er Sémestre (t. liv. No. 16), p. 876.

(d) M. Delaunay adopte la correction que j'ai faite au terme de M. Plana, quoiqu'il ne l'ait point remarqué dans son tableau.

(e) Plusieurs combinaisons paraissent avoir été omises par MM. Plana et Delaunay dans la formation de ce terme.

(f) Le coefficient de notre théorie différait légèrement de celui de M. Delaunay, la rectification d'une erreur de calcul a ramené l'accord.

Valeur Inverse du Rayon Vecteur.

M. Delaunay n'ayant porté l'approximation dans l'expression de la valeur inverse du rayon vecteur que jusqu'aux quantités du 5e ordre, et ses résultats se trouvant *à une seule* exception près parfaitement d'accord dans cette limite d'approximation, avec ceux de ses dévanciers, nous n'aurons de notre coté que de très courtes observations à présenter sur cet article.

Argument.	Terme de Plana.	Terme de Delaunay.	Théorie Analytique.
l	$-\frac{152129}{4608} m^4 e$	$-\frac{153389}{4608} m^4 e$	$-\frac{152129}{4608} m^4 e$

Observation.

Le coefficient du terme multiplié par cos l dans l'expression du rayon vecteur, se déduit du coefficient du terme correspondant ou du terme multiplié par sin l dans l'expression de la longitude, on ne conçoit donc pas la correction proposée par M. Delaunay relativement au coefficient de ce terme dans l'expression du rayon vecteur puisqu'il admet le coefficient du terme correspondant dans l'expression de la longitude, tel que le donne M. Plana.

J'ai reconnu en outre, que conformement à l'observation de M. Delaunay, les 3 termes suivants devaient être ajoutés à ceux que j'avais déjà signalés comme devant être supprimés dans l'expression de la valeur inverse du rayon vecteur donnée par M. Plana :

$$\text{Arg. } 2\,D - l - l' \qquad + \frac{35}{8} m e^3 e'$$

$$\text{Arg. } 2\,D - l + l' \qquad - \frac{15}{8} m e^3 e'$$

$$\text{Arg. } 2\,D - 2\,F \qquad + \frac{3}{32} m \gamma^4$$

Il résulte de la discussion précédente, 1°, Que quelques unes des corrections apportées par M. Delaunay aux résultats de M. Plana, même en se bornant à l'ordre d'approximation auquel il s'est arrêté paraissent inéxactes ; 2°, Que les différences

entre les coefficients des diverses inégalités lunaires donnés par ces deux géomètres et ceux qui se déduisent de notre théorie, se réduisent en définitive à un petit nombre de termes sans importance par leur valeur numérique, et qu'on ferait du reste aisément disparaître, soit en reprenant les calculs de M. Plana, ce qui est facile puis qu'il en a donné tout le détail dans son ouvrage, soit que M. Delaunay se décide lui-même à revoir quelques uns de ses résultats avant de les livrer à l'impression. Quoiqu'il en soit, on peut régarder comme un fait très important dans la théorie du système du monde, que cette coincidence à-peu-près complète des formules littérales qui déterminent *la longitude, la latitude,* et le *rayon vecteur* de la Lune, obtenues par trois méthodes absolument différentes et en portant l'approximation aussi loin que la pratique pouvait l'exiger. Ce résultat que les géomètres du dernier siècle n'avaient osé espérer, doit puissamment contribuer au perfectionnement des tables lunaires, et donner à leur construction toute la précision qu'ont acquise nos autres tables astronomiques.

Further Observations on Solar Spots.

By the Rev. F. Howlett.

It is with much pleasure that I submit to the indulgent attention of the Royal Astronomical Society a further account of observations of the Solar Spots; begging the Society to accept also the fruits, whatever they may be worth, of about eighteen months' additional honest endeavours to depict certain of those interesting phenomena which still continue to perplex the astronomical world, strongly prepossessed though some observers may be as to the theory of their character and import.

I have now the honour of laying before the Society a further series of drawings contained in about 64 sheets, comprising, first, some 128 whole disks on a scale throughout somewhat smaller than that usually employed hitherto by heliophotographers, and giving about 1 inch to $9' 37''$ of arc. The sheets exhibit, also, secondly, several hundreds of drawings of spots and groups of spots, on a scale varying from 1 inch to about $115''$, to the considerably larger scale of 1 inch to $62''$; which last is, perhaps, as convenient and instructive a size as could well be adopted for representing the details of the solar phenomena generally, in a daily record.

As I stated at our Monthly Meeting in December 1862, the amplification last mentioned may be readily and very effectively obtained by projecting the Sun's image in a duly darkened

chamber, upon a cardboard or other screen, placed 4 ft. 2 inches from the eye-piece of a telescope (of 3 feet 8 inches focal length and $3\frac{3}{4}$ inches aperture), the eye-piece magnifying about 80 times linear, and containing a kind of micrometer, or divided-glass-diaphragm, ruled off into $\frac{1}{100}$ths of an inch, each of the divisions of which is also seen distinctly projected upon the screen, and may be brought to correspond exactly with the several half-inches of a foot-rule carefully drawn upon the screen in ink.

By the above adjustment every $\frac{1}{16}$th of an inch would, of course, represent about 4″ of arc upon the projected solar disk.

When an eye-piece of 150 is employed, and the distance of the screen therefrom is adjusted to 3 feet 8 inches, every $\frac{1}{16}$th of an inch represents 2″ of arc only; which amplification is likewise attainable by employing a power of 120, and removing the screen to a distance of 5 feet 6 inches.

I have not yet estimated the value of still higher powers in connexion with the screen, though I occasionally scrutinise the spots by their aid.

A circle of about 18 inches in diameter, marked off into 360 degrees, is also drawn upon the screen, and a plumb-line hangs before it in order to insure the perpendicularity of the line joining the degrees 0° and 180° (or any other line parallel thereto), which represents the zenith and nadir points, for the time being, of the projected solar image.

When the screen is nicely and squarely adjusted (on an easel or otherwise) before the telescope, a fiducial line showing the course that is being pursued by a spot at any given hour across the disk, may be very approximately obtained by observing at what degree any spot enters, and at what degree also it quits, the circle, and referring this line, at leisure, by a parallel-ruler to the centre of the small drawing of the solar disk for the day; upon which drawing, however, the *positions* of the spots should be carefully dotted down immediately after noticing the course pursued by any one of them across the screen. The details of nuclei and penumbræ, &c. may of course then be drawn more leisurely. But a better plan is to throw any small, neatly defined spot precisely upon the central point of the circle on the screen, and observe where it cuts the circumference, which at once (not reckoning, however, the reversion of the image) gives the radial half of the fiducial line traversed by the spot. Despatch in noting down the positions of the spots is the more imperative the nearer it may be to the hour of noon that the observation is made; whilst at about an hour and a half after 6 A.M. and the same time before 6 P.M., the positions may be taken very deliberately—the line of junction of any two spots appearing then to be nearly stationary—though this line would be seen to retrograde before 6 A.M. and after 6 P.M. Indeed, since this

paper was commenced, I have been assured by an observer,* who informs me that he went in the year 1861 to high northern latitudes with the view of taking notes in connexion with astronomical subjects (chiefly solar), that he has, when within the Arctic Circle, actually seen this retrogradation going on during the whole period between 6 P.M. and 6 A.M.: the rate of retrogradation increasing or diminishing according as the hour was nearer to or further from the period sometimes called the "midnight sun." (See also the subject alluded to in the *Monthly Notices*, vol. xxi. No. 2, page 50, for December 14th, 1860.)

My observations, and some also of my drawings of the very conspicuous group of Solar Spots which passed off the disk on the evening of the 4th August, 1862, have already been honoured with a place in the *Monthly Notices* (vol. xxiii. No. 3), and the only reasons why I would wish to allude to them again on this occasion are these:—In the woodcut, fig. 6, p. 113, owing to some mistake, an actual notch in the Sun's normal or average margin is represented in the figure; whereas, in point of fact (and this is a very important difference), what I observed and described was only a notch caused, apparently, by an abnormal heaping up of facular photospheric matter to the north and south of the position occupied by the notch P.

Then, again, I must beg permission to demur to a statement made by Herrn Spoerer in their elaborate and careful paper in the *Astronomische Nachrichten* (No. 1402, column 147), wherein they assert that by mid-day on August 4th, 1862, the penumbra of the large spot (numbered by them 79†) "extended to the Sun's extreme edge." Whereas, owing in part probably to the very serene state of the atmosphere at my station of observation, I was enabled, at the hour above mentioned, to discern an unbroken streak of photosphere between the western edge of the penumbra and the Sun's limb; and as late even as 3.50 P.M. to estimate the breadth of this streak at about 2″.

By 6 P.M. indeed, and possibly at an earlier period, I observed that the nucleus of the great spot (79 of Prof. Spoerer, and ϱ, August 4th, in my record) had entirely disappeared, portions of penumbral matter, however, reaching completely to the Sun's extreme margin: and this has been the only occasion on which I could positively assert that I have seen the penumbral matter on the very edge of the Sun, with the exception of the interesting group $\varkappa$, which appeared upon the disk on the morning of February 18th, 1863, and included many patches

* Mr. J. Macnider, of No. 10 Norfolk Street, Strand.

† The diameter of this spot is reckoned by Prof. Spoerer at 4140 geographical miles, which space on the solar surface would subtend an angle of little more than 9″, whereas it was really at least 85″ in diameter when seen about the middle of the disk, as on July 29th, which is equivalent to fully 38,000 geographical miles.

of penumbra, and several strongly marked faculæ, and to which I shall have occasion to allude again in the present paper.

The usual preponderance of spots in the northern hemisphere has been as apparent as ever during the eighteen months last past. Out of the 128 disks now on the table it may, I think, be shown that a northern preponderance obtained on 80 days, a southern on 43, whilst on the remaining 5 days the spots were distributed pretty equally (more particularly as regards their energy) on each side of the Solar equator. Spots prevailed in the northern hemisphere during the whole of November and December 1861, as also during the whole of January, and first half of February 1862. My observations were suspended until about April 12th, 1862, when the spots were likewise in force in the north. They then went on alternating with tolerable impartiality (so far as I observed them) till about the last week in July 1862, when the northern preponderance was again apparent till the middle of August. During the whole of September, however, the south was chiefly in activity. For the first half of October the north again had the advantage; but during the next seven weeks I was unable to continue my record, having been absent from England; though, on my return home in the first week of December 1862, I found that the north was once more in the ascendant, and so it continued till about January 12th, 1863. Southern spots then prevailed till about February 18th. They then alternated (or seemed nearly evenly distributed at times) till March 17, after which northern spots maintained their ground till April 7th. The south then showed a superiority for about a week or so, then the north for a fortnight, and then the south once more resumed the preponderance, which it continued to hold up to May 19th, 1863, when the northern spots, though sufficiently important, were far excelled in interest and number by the southern ones; after which the northern spots altogether predominated till the end of May 1863. As, however, it is by no means an easy matter to determine always the precise line of the Sun's equator or of his parallels of latitude, I offer certain of the above particulars subject to correction.

During the past eighteen months the only day on which the Sun was observed by me to be devoid altogether of spots was the 9th April of the present year. I did not, however, scrutinise the disk very closely on the day in question, and certainly a small nucleated spot was visible on the 10th, about one minute from the Sun's preceding limb, and possibly, therefore, it may have been in existence on the day previous.

As a general rule spots have been tolerably plentiful on most days ever since the year of their estimated maximum in 1859; and their minimum period must not be expected before 1865 or 1866; reckoning, that is, from data collected by M.

Wolf, of Berne, and cited by Sir John Herschel in his *Outlines of Astronomy* (Ed. 1858), section 394[a].

There is a peculiar curvilinear arrangement of minute spots and patches of penumbræ, generally concentric, with a larger nucleated spot or group of spots, which I have not unfrequently noticed; and which, as well also as a highly interesting double, or sigmatoid curve, assumed sometimes by the little subordinate spots and patches, would seem to bear strongly upon the theory of cyclones upon the Sun's surface. These phenomena are recorded in sheets 73, April 30th, and 77, June 4th, 1862; in 99, Feb. 16th; 110, April 19th; 111, April 21st; and 115, May 11th, 13th, and 15th, 1863.

By the use of the screen I have obtained a far better insight than I could otherwise have had into the appearances and general disposition of those remarkable solar phenomena, the Faculæ. As is well known, these rarely or never occur in either high northern or southern latitudes, though they abound commonly in the zones occupied by the spots. Their forms are very varied, and often very fantastic. Sometimes they exist as small amorphous patches; sometimes they are stellate; at other times they assume a kind of reticulated or anastomosing appearance, or (as Profs. Spoerer observe) that of the circular lunar craters. Not unfrequently I have noticed them closely resembling the "merry-thought" of a bird, or even grotesque attenuated human figures. These appearances may be noted in the order in which they are mentioned in sheets 106, 122, 107, 109, 108, 114, &c.

The Faculæ are not only to be seen when near to the eastern or western limb of the Sun, but often also when quite on the central portions of the disk, where, if I interpret rightly certain appearances frequently presented on the screen, their existence may, amongst other forms which they are apt to assume, be detected as narrow, irregular, faintly luminous borderings, from about 4″ to 8″ in width, immediately surrounding *some*, though not all of the larger spots; and this luminosity appeared to me to be caused by an extra thickening of the photospheric matter by which the spots are surrounded, and by the absence, as a natural consequence, of that peculiar porous, curdled, and comparatively dusky appearance (caused in all probability by the interstices between the so-called "willow-leaves," or perhaps small aggregations of the willow-leaves of Mr. Nasmyth) which characterises the whole of the solar surface not actually occupied by the spots or faculæ. Moreover, I do not think that the faint borderings round the spots above alluded to are the effect of any instrumental or optical illusion as I at first rather suspected.

A representation of their general appearance may be seen in sheet 99 of my record, under date Feb. 16th, 1863.

There is another point which may perhaps have been

noticed by other observers, but which I think well merits attention in any theorisings upon the constitution of the Sun: I allude to the frequently remarkable *persistency* of some of the smaller and more isolated spots, whether situated either more remote from or nearer to the Sun's equator. Whilst the contour and the details of the larger groups may be seen frequently to undergo a complete transformation during their passage across the disk, so that *hundreds of millions* of square miles of spots and penumbral matter are either entirely disposed of or so changed and re-arranged that it becomes impossible to recognise them under their new forms, it often happens, in the case of some of the smaller isolated spots, that one of not more, perhaps, than 5″ or 6″ in diameter (which small quantity, however, on the solar surface would imply a superficial area of from five to seven million square miles*) will be seen to maintain a nearly unaltered appearance for several consecutive days.

I have on previous occasions given in my humble adherence to the theory that the Solar Spots are cavities in the surface of our luminary, the nuclei being the most deeply sunk portions, the penumbra commonly shelving down to them on all sides from the external photosphere; though at the same time I had generally found that the penumbræ possessed only an exceedingly slight amount of depression within the solar surface. Very palpable evidence of this latter fact was afforded, as it seemed to me, by the fine group π of the 18th Feb. 1863, already mentioned (sheet 99). The group in question consisted of two somewhat contiguous large spots, accompanied, as it afterwards proved, by many faculæ and numerous smaller spots, and detached patches of penumbra. But so broadly conspicuous (as seen projected on the screen) were the nuclei of both the contiguous large spots, and so completely devoid was the most westerly nucleus of *any measure* of penumbra on its eastern (following) edge, that I not only was more strongly convinced than ever of the extreme shallowness of the penumbral depressions, but I even for a while wondered whether, after all, they and the nuclei could be depressions at all!

The two nuclei were distant, respectively, about 15″ and 30″ from the Sun's limb at the moment when I experienced these doubts; but when the group was well advanced upon the disk (as it was by Feb. 21, when I next observed it), it was evident that I could not have seen any penumbra upon the eastern side of the great westerly nucleus, for the simple reason that it proved to be devoid of any in that direction;

* 1″ subtends 450 miles. Therefore,

$$(5'' \times 450)^2 = 5{,}073{,}750 \text{ square miles}$$

and,

$$(6 \times 450)^2 = 7{,}290{,}000 \quad ,, \quad ,,$$

whilst conversely the easterly large spot proved to have but comparatively little penumbra upon its western (preceding) side, and what little it had would doubtless have been still further diminished to sight on the 18th by the effect of foreshortening, as first described by Dr. Wilson. It was on this same day (Feb. 18th, 1863) that I observed, for the second time only within my experience, that the penumbral matter in straggling connexion with the most easterly spot of group π extended absolutely to the Sun's edge, and surrounded, evidently, the northern extremity of a conspicuous mass of faculæ which extended along the limb of the Sun; many other facular streaks also occurring in the neighbourhood of the spots, and extending uninterruptedly to the great group lettered ϵ, which lay about 2′ 25″ to the north.

A group possessing many features in common with the one just described as group π came upon the disk on the morning of April 12th, 1863, and in neither of the two principal spots was I able to trace much appearance of penumbra on the eastern side of their respective nuclei; but the details of the group had suffered such extensive changes by April 15th that the appearances they exhibited, when well on the disk at the latter date, could by no means be appealed to for evidence as to what might have been the disposition of the various nuclei and penumbræ on the 12th of the same month. I had observed these changes going on during the interim, but was unable to secure drawings of them. I particularly noticed, however, that the most westerly spot, lettered η 5, had shrunk to about one-fourth of the magnitude it had possessed on entering the disk.

Since the Meeting in May last I have observed and drawn an unusually interesting group, as shown in sheet 115, under date May 11th, 1863, and letter ξ of my record. Both the principal spot, as well as from twenty to thirty much smaller subordinate ones, appeared to be obeying some strongly vorticose impulse during this period of their development. Starting either from a small group of faint penumbral patches situated about 45″ to the east, or more probably, as I thought, from a more considerable spot about 18″ in length, lying 12″ or so to the north-east of the principal spot, they had formed around this latter for fully half the circumference of a circle, adopting a curious, bifurcating, diverging course, bearing no inconsiderable resemblance to the spiral structure of some of the great nebulous clusters of stars, as revealed in Lord Rosse's gigantic reflecting telescope.

I did not see the group ξ again until the morning of May 13th, when its whole appearance was as much changed as could well be conceived. The small detached eastern group above alluded to had developed into a large elongated trifid connected spot, fully 1′ 17″ in length from north-west to south-

east, and containing three or four easily-discerned nuclei; whilst the other series of small spots (which had presented, two days previously, that peculiar spiral arrangement) were replaced also by a large, very irregular confluent group, about 1′ 30″ in length, likewise extending from north-west to south-east, and comprising six or eight small nuclei, which were represented by three very much larger ones on the day following (May 14th), when I last observed them.

Moreover, on May 13th, a line of faint and somewhat remotely separated penumbral specks swept through the midst of group ξ in a delicate sigmatoid curve (very similar in its character to the sigmatoid curve observed by me on April 30, 1862), which appeared to indicate the existence of some common and extensive influence then at work throughout the whole system of the aggregate group, and which curve may also be seen to bifurcate at a point not far from equidistant between the principal spot and the north-west extremity of the trifid easterly group.

It is also, perhaps, worthy of note that, whilst from these and some other of my drawings it is apparent that the nuclei sometimes preceded the formation of their penumbræ, at other times the reverse was the case; for I presume that the illustrious Kirchhoff would by no means wish to assert that the former case always holds good, as one might almost seem to gather from a passage (p. 27) of Prof. Roscoe's translation of Kirchhoff's *Researches on the Solar Spectrum and the Spectra of the Chemical Elements.*

I have only one more subject to mention, and that is, that I hope some one else besides myself took notice of and depicted, or, better still, secured a photograph of, a most curious phantom-looking group of spots which, at 1 P.M. on the 4th January of this present year, exhibited an appearance so wonderfully like a human skeleton, that, in a less superstitious age than the nineteenth century, its portentous shape might easily have raised considerable apprehension in the minds of the multitude.

Being Sunday when this was observed, and being much occupied with the more immediate duties of my profession, I did not draw the group with micrometric correctness, but simply took a rough sketch of the phantom, which subtended about 5′ 40″ of arc, or 153,000 miles, and respecting which (as I observed lately to Admiral Manners) I am really not aware that any love for the marvellous induced me to exaggerate in any degree the singularity of its proportions.

Sheet 96 exhibits this group as it appeared, when much altered, on Jan. 7th.

On Eyepieces.

(*Extract of a Letter from the Rev. W. R. Dawes.*)

I have seen enough of the three eyepieces you sent to report upon them with some confidence.

1st. The *Airy* (marked with pencil 271).* I cannot believe this to be a fair representation of the proper combination of curves pointed out by Mr. Airy. The field is by no means large, and towards the edge there is a very decided red fringe. It is vastly inferior to any ordinary astronomical Huyghenian eyepiece I possess, as constructed by Dollond, Ramsden, and Tulley. The angular subtense of the diaphragm is 37° 57′, and of that small angle not more than about 25° are good.

2. The *Kellner* (as it professes to be) is also an unfair specimen of his construction, the diaphragm being *considerably too small.* The angular subtense is only 37°·35; but the distinctness at the very edge of the field is scarcely less than in the centre, and gives the impression that the angular subtense of the diaphragm might be advantageously enlarged to at least 48° or 50°.

3. The *Aplanatic* has a very large field, and is pleasantly *achromatic* to the edge; but the exterior 4° or 5° of the angular subtense of the diaphragm (which = 59°·47) are far from distinct; and I doubt whether much more than 50° out of the whole subtense would be sufficiently so. To determine this, however, with exactness, would require careful observations on a fine night; and I have not been able to spare the *fine* starlight for the purpose. This eyepiece has a double ghost when a bright object is in the field.

An ordinary *Ramsden* Huyghenian which I have has a perfectly distinct and achromatic angular subtense of 48°.

In a *Kellner's* eyepiece, *made by himself*, the angular subtense = 49°·56: admirably distinct and achromatic to the edge.

In a *Dollond* Huyghenian, the angular subtense = 46°·46: nearly perfect throughout.

In a *Tulley* Huyghenian, the angular subtense = 51°·84: extremely fine in colour and distinctness.

These, you will see, are scarcely, if at all, inferior to the "*Aplanatic*," and have far less of ghostly appendages.

Hopefield Observatory, Haddenham, Thame,
April 9, 1863.

* The Astronomer Royal remarks that he has never seen this eyepiece, and knows nothing about it.—Ed.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1863, *TO JUNE* 1864.

VOL. XXIV.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS
OF THE ROYAL ASTRONOMICAL SOCIETY.

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Complete

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIV. *Nov.* 13, 1863. No. 1.

Dr. LEE, Vice-President, in the Chair.

The Rev. Chadwick Bates, Manchester;
Wm. Martin, Esq., 32 Aylesford Street, St. George's Square; and
A. C. Ranyard, Esq., 13 Hunter Street, Brunswick Square,
were balloted for and duly elected Fellows of the Society.

M. Léon Foucault; M. Kowalski; M. Winnecke; and Prof. G. P. Bond, were balloted for and duly elected Associates of the Society.

In the absence of the Astronomer Royal, it was announced to the Society by the Chairman, that a convention had been arranged between M. Le Verrier and the Astronomer Royal for dividing the labour of meridional observations of the Small Planets, and of the Planets generally. The peculiar form given to the agreement is determined by the special responsibilities of the Observatory of Greenwich. Since the foundation of that Observatory, incessant observations of the Moon have been considered as its most sacred object. While the meridional observations of the Moon occur in the evening, the addition of Planetary observations only adds to the labours of the Observatory in proportion to the number of observations; but,

during the observations of the morning Moon, the evening observations of Planets add a distinct labour of oppressive character. For the morning and evening meridional observations, and for the altazimuth observations, with alternations of rest to the observers, it is then necessary to allocate six observers. In order to diminish this oppression, it has been proposed by the Astronomer Royal, and most liberally agreed to by M. Le Verrier, that the Observatory of Paris should undertake the Planetary observations from Full Moon to New Moon, the Observatory of Greenwich remaining charged with those from New Moon to Full Moon.

In regard to the extent of observation of each Planet, it is understood that at the first, second, or third appearance of each Small Planet, it is to be observed as often as possible; but, with that exception, the Small Planets, and also *Uranus* and *Neptune*, are to be observed only between 10^h and 13^h solar time. The observations of *Mars*, however, are to begin when he passes at 15^h, and to be continued to quadrature, those of *Jupiter* and *Saturn* to begin at passage at 14^h and to be continued to about 8^h.

The Astronomer Royal cannot too strongly express his feelings on the zeal, the liberality, the ardour for science, and the personal kindness to himself, with which his advances have been met by M. Le Verrier.

The Astronomer Royal also thinks it a subject of congratulation that an example has been set of the combination of the efforts of different Observatories. There can be no doubt that a great amount of labour has been wasted for want of such arrangements; and if they could be further extended, the energies of many observers would be set free for important objects which hitherto have failed to receive sufficient attention.

The Astronomer Royal solicits the attention of individual observers to a request, originally circulated by himself, but now republished by M. Le Verrier in his daily *Bulletin*, to the effect that every observer, who has ascertained the error of any of the Ephemerides of the Small Planets, would immediately communicate it either to M. Le Verrier or to the Astronomer Royal. In either case it will be immediately published in the *Bulletin*. The knowledge of the amount of the error of Ephemeris very greatly facilitates the discovery and observation of the Planet with meridional instruments.

Notes on η Argus. By F. Abbott, Esq.

The anomaly connected with this remarkable Star, as to any fixed period of increase or diminution, still remains in-

explicable. Whether it will eventually disappear altogether or not is a problem yet to be solved, or whether its apparent variability is produced by changes in its own nature, or caused by other surrounding objects, is still open to discussion.

Since my former notice of this Star (April 13th, 1861) there has been a considerable diminution both in its size and light, but at no regular intervals, as will appear by the tabulated record accompanying this paper. There are circumstances, however, connected with the registering of η *Argus*, from time to time, together with the nebulous matter surrounding it, which will go far to show that not only is the Star variable, but that all the objects encircling it are subject to change.

In comparing η *Argus* with other stars it was found necessary to use more than one star of comparison, and also to consider the nature of the light at the time of comparison. Much depends upon whether the observation is made in twilight, moonlight, or starlight. η *Argus* by twilight was more difficult to find by the naked eye than any of the comparison stars; but when once it was lighted up by the surrounding Nebulæ, the eye caught it much sooner than other stars of the same magnitude.

Why it is necessary to use more than one comparison star in each observation is in case the star of comparison itself might be variable, as the following extracts from my register will illustrate.

Sept. 24th, 1861. Eve brilliant, fourteen stars, some close double well separated. By moonlight or twilight, when the nebulosity surrounding η *Argus* is destroyed, the star is identical both in size and light with λ *Centauri*.

Jan. 17th and 18th, 1862. With the Moon at full, and at the same altitude as η *Argus*, the star was still equal (by the naked eye) to 943 Lac., and λ *Centauri*, but with a low magnifying power η *Argus* was quite ruddy, as were all the stars in the vicinity.

July 2d, 1862. The star marked 943 Lac., in *Argo Navis*, is no longer a star of the 4th magnitude. In comparing η *Argus* with that star, together with *q* Lac., 11 *Argus*, *t* 1 *Argus*, and 926 *t*., all stars of the 5th magnitude, no difference could be seen.

That the duration of this Star's apparition is variable to a great extent is certain; and by comparing the present description with the monograph of Sir J. Herschel, taken at Cape of Good Hope, it will, I think, appear conclusive that the apparition of the surrounding Nebulæ is also variable.

Messier recommended careful observations to be made on such objects, with a view to ascertain whether or not any indications of change in form or structure are exhibited. That such changes have taken place is already on record. An instance is afforded by the remarkable Nebula surrounding θ

in the constellation of *Orion*, which Huyghens discovered in 1656, and noted in his *Systema Saturnium;* from comparisons of which Sir Wm. Herschel, by his own observations from 1783 to 1811, concluded it had undergone sensible change. Bouillard and Le Gentel maintained the same opinion in reference to the Nebulæ in *Andromeda;* and of later date, Bond, Pogson, Struve, D'Arrest, and others, have observed such changes.

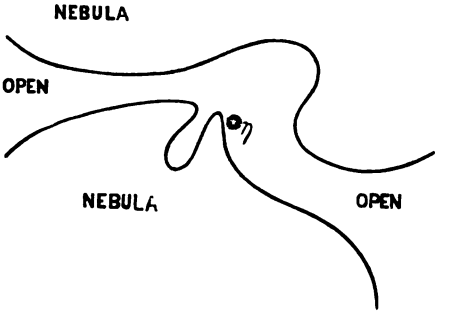
Sir John Herschel, when at the Cape, carefully examined η *Argus* with an 18-inch reflector; "No part of this Nebula," says Sir John, "shows any sign of resolution into stars." "It is not easy," he adds, "for language to convey a full impression of the beauty and sublimity of the spectacle which the Nebula offers as it enters the field of the telescope." "It will appear," he also writes, "that every succeeding state of nebulous matter is the result of the action of gravitation, and by such steps the successive condensation of it is brought to a planetary or stellar condition. Several instances are on record which connect the planetary with the stellar appearance. In those instances wherein the collection of nebulous matter was very extensive, subordinate centres of attraction could not fail to be established, around which the adjacent particles would arrange themselves, and thus the whole mass would in process of time be transformed into a determinate number of discrete bodies, which would ultimately assume the condition of a cluster of stars."

That this condition is partly carried out in the object η *Argus* will be manifest by comparing the Cape description with the present one. A great difference may be caused by the optical means employed, as far as resolvibility goes; but if an increased number of brilliant isolated stars, with a change in position of η *Argus*, is the effect produced with a 5-foot achromatic, and they were not exhibited to the same effect in an 18-inch reflector, it only strengthens the evidence in favour of some change having taken place in the object.

The open space, as given in the Cape Monograph, and also in the last edition of the *Outlines*, is somewhat in the form of a dumb-bell, compressed in the centre and surrounded with Nebulæ, in the most dense part of which is situated η *Argus*. The appearance of the open space now assumes the form of a crooked billet, wide in the centre and open at both ends, with η *Argus* situated within the open space or dark part, and surrounded with an almost innumerable quantity of brilliant stars, many of which are arranged in groups, some being of a blue, and some of a ruddy colour. They are remarkably brilliant in the dark space, and afford a good comparison with the variable Star itself.

It appears somewhat paradoxical that in 1838, when examined by Sir J. Herschel, the star η *Argus* was situated in the most dense part of the Nebula, and was seen as a star of the first magnitude. And now in 1863, it is out of the Nebula,

and within the dark space it only appears a star of the sixth magnitude.



That the star's Right Ascension has not varied so much will be manifest. It is clear then that the dense portion of the Nebula, towards the east, must have receded, leaving each end open, and η *Argus*, together with about 70 stars ± up to the 14th magnitude, as seen within the dark space.

The irregularity of this Star, and the nebulosity surrounding it, involves a principle as to whether its accession and diminution is the effect produced by distance, transits of opaque bodies or solar spots; or whether the nebulosity surrounding η *Argus* interferes with the light emitted by the star; if so, the increase and diminution, however vacillating, become obvious.

Record of Observations.

Observed Magnitudes of η *Argus*. H. 3295; Lac. 968; R.A. $10^h\ 39^m\ 45^s$; N.P.D. $148^\circ\ 57'\ 56''$. Epoch 1863.

Year.	Month.	Observer.	Mag.	Remarks.
1677	,,	Halley	4	
1751	,,	Lacaille	2	
1811–15	,,	Burchell	4	
1822	,,	Fallows	2	
1827	,,	Burchell	1	Equal to α *Crucis*
1828	,,	Do	2.1	
1829–33	,,	Johnson	2	
1832–33	,,	Taylor	2	
1834–37	,,	Herschel	1.2	
1838	Jan. 2	Do.	1	Equal to α *Crucis*

Year.	Month.	Observer.	Mag.	Remarks.
1842	Mar. 19	Maclear	1	Inferior to α *Crucis*.
1843	April	Do.	1	Nearly equal to *Sirius*.
1843	Apr. 11	Mackay	1	Fully as bright as *Canopus*.
1850	,,	Gillis	1	Equal to *Canopus*.
1856	,,	Abbott	1	Equal α *Centauri*.
1858	Feb. 10	Do.	2	Equal β *Crucis*.
1858	Apr. 2	Do.	2	Equal γ *Crucis*.
1858	May 28	Do.	2.1	γ *Crucis* brighter of the two.
1858	Oct. 8	Do.	2.4	γ *Crucis* much superior.
1859	Mar. 27	Do.	2.5	Much inferior to ι *Argus*.
1859	July 6	Do.	3	Equal to κ *Argus*.
1860	Nov. 2	Do.	3.1	Inferior to δ *Crucis*.
1861	Mar. 21	Do.	3.5	Very inferior to δ *Crucis*.
1861	Apr. 8	Do.	4	Equal to ι *Crucis*.
1861	Apr. 13	Do.	4.1	Inferior to ι *Crucis*.
1861	June 19	Do.	5	Equal to 868 *n Argus*.
1861	June 30	Do.	5	868 *n Argus*, seen in twilight before η *Argus*.
1861	July & Aug.	Do.	4	No difference between φ and η *Argus*.
1861	Sept. 24	Do.	4	Equal to λ *Centauri*.
1861	Oct. 12	Do.	4	Equal to λ *Centauri*.
1861	Dec. 21	Do.	4	Equal to either λ *Centauri*, or 943 P *Argo Navis*.
1862	Jan. 17 & 18	Do.	4	Equal to λ *Centauri*, or 943 P. *Argo Navis*.
1862	Feb. 12	Do.	4	No difference from the two former, either by twilight, moonlight, or starlight.
1862	June 14	Do.	4.5	Not so large as 943 P., and larger than *q*, close stars.
1862	July 2	Do.	5	Equal to ι *Crucis*.
1862	Aug. & Sept.	Do.	5	Equal to ι *Crucis*.
1862	Nov. 14	Do.	5	For the last five months η *Argus* has not exceeded the 5th magnitude. Below the Pole, or by moonlight, it is scarcely visible as a star to the naked eye.
1863	Mar. 30	Do.	6	Equal to *z* and 977 Lac.
1863	Apr. 23	Do.	6	Fine clear night. Star in the zenith; by the naked eye scarcely recognised, as a star. With a low power it was equal to 951, 954, and 977, Lac.
1863	May 23	Do.	6	A drawing made of the object. η *Argus* quite distinct within the dark space which is open at both ends.

Private Observatory, Hobart Town,
June 1863.

On the Colours of the two Component Stars of 95 *Herculis.*

Mr. Higgens, in a letter dated Bedford, 17th August, 1863, addressed to Admiral Smyth, writes as follows:—

"I beg to forward a few notes on 95 *Herculis.*

"When I last saw the star, in the autumn of last year, the colours were as noticed in the *Cycle:* A, apple-green; B, cherry-red.

"I first saw it this year on the 23d April, but the bright hues were not there, and greenish-white and pinkish-white were all I could affirm.

"May 10th. Hues more faint. I could only record them as dull white, both A and B.

"Aug. 1st. A, greenish-white; B, yellowish: both changing nightly till Aug. 12, when they showed as in the *Cycle:* A, apple-green; B, cherry-red. A first showed signs of deepening colour, the hues becoming more apparent every night, B changing from yellow to red more rapidly.

"It was not possible to note night by night the amount of change; but it was very palpable after an interval of three or four nights, and continued so till both stars showed their normal colours.

"The instruments used were, a $3\frac{1}{4}$-inch achromatic, with 80, and a 4-inch with 115; both glasses by Cooke, of York."

Admiral Smyth remarks:—

"The star in question, 95 *Herculis,* is now a crucial instance of sidereal colour-changing; and it has given some little trouble, both to my son Piazzi and myself already, in poring over the registered observations.

"It is to be hoped that a field at once so elegant, easy, and useful, will be followed up by some of those Fellows of the Society who possess both means and leisure to pursue the interesting inquiry."

The Astronomer Royal has, however, suggested to Admiral Smyth that the simultaneous change for the two stars is suspicious, and looks like a possible change in the telescope.

On the Nebula of Orion. By Dr. Winnecke.

(*Extract from a Letter to Mr. De La Rue.*)

M. Struve's observations, published in the "Memoir on the Great Nebula of *Orion,*" indicate with high probability the variability of light of some of the very feeble stars in the immediate neighbourhood of the Trapezium. Supposing that

this part of the heavens would be examined by M. Struve with the Malta Telescope, I have made, a few weeks ago, some observations on the Nebula with our Refractor. M. Struve has given (page 118 *et seq.* of his Memoir) a Catalogue of all the stars, noticed by former astronomers, which have been certainly made out with the Pulkowa Refractor. It is, therefore, very curious, that I have recently discovered five stars in the neighbourhood of the Trapezium, which are wanting in the general Catalogue mentioned above. You are acquainted with the extreme sharpness of M. Struve's eyes, and you will be with me certainly of opinion, that stars, which I measured with the micrometer, could not have been overlooked by him, had their brightness been unaltered. Still, it must be mentioned, that all these stars are to be found on Mr. Lassell's map, and probably also on the delineation of the Nebula given by Mr. Bond; a fact which I did not know when I first saw them.

The five new stars are the following:—

Comes of *h* 81	Dist. = 24″	Angle of Pos. = 107°·4
Comes of *h* 54	Position from *h* 50	115°·3
	,, from *h* 53	12°·7
Comes of *h* 75	Dist. = 9″ (est.)	Angle of Pos. = 33°·8

Two very small stars, north preceding the Trapezium; the co-ordinates of which related to the principal star *θ' Orion*, I estimate

$$\Delta\alpha = -21'', \qquad \Delta\delta = +24'',$$

and

$$\Delta\alpha = -15'', \qquad \Delta\delta = +23\tfrac{1}{2}''.$$

The angle of position of the following from the preceding was measured = 91°.

I should feel very glad if you would communicate this short notice on the feeble stars to the Royal Astronomical Society.

Pulkowa, Nov. 3, 1863.

Calculation of the Sun's Parallax from the Lunar Theory. By P. A. Hansen.

(*Translated by W. T. Lynn, Esq., of the Royal Observatory, Greenwich; Communicated by the Astronomer Royal.*)

The determination of the Solar Parallax has become lately a problem of special importance; as several investigations thereof, made in totally different manners, have given values, agreeing well among themselves, but differing considerably

from the value hitherto assumed. While, on the one hand, the Sun's Parallax had, by means of the two transits of *Venus* in the last century, been found to be $= 8''\cdot56$, and this value has been generally used; on the other hand, the recent determinations, differing very little from each other, give $8''\cdot9$, or $\frac{1}{25}$th greater. Under these circumstances it is of especial interest to discover what value the Lunar Theory gives for the Solar Parallax; and this is so much the more important because its determination by means of the Lunar Theory is one of the most certain determinations of it which exists. The accuracy of the result depends principally upon the accuracy with which the coefficient of the so-called parallactic inequality can be determined by observations; and an error in this affects in a greatly diminished proportion the Sun's Parallax; for an error of $0''\cdot1$ in the determination of this coefficient produces an error of only $0''\cdot007$ in the Solar Parallax deduced from it. I have, therefore, in the elaboration of the Lunar Tables, kept this point in view, and, at the same time with the determination of the corrections of the assumed elliptic elements of the Moon's orbit, have determined the corrections of all the lunar perturbations dependent upon the Solar Parallax. That this has given me an augmentation of the hitherto assumed Solar Parallax, I have already announced in the year 1854, in the *Monthly Notices*, vol. xv. No. 1, p. 9.* In the same Periodical, vol. xxiii. No. 8, I have briefly given the Solar Parallax thus obtained; but, for the reasons mentioned above, I will here give a more accurate statement, which I extract from the manuscript of my second treatise on the calculation of the lunar perturbations, the printing of which will soon commence.

It is known that in the Moon's motion a number of inequalities arise, which are to be multiplied by the factor $\frac{a}{a'}$, that is, by the proportion of the semi-axis major of the lunar orbit to that of the Sun's orbit (Earth's orbit). The accuracy with which these inequalities can be theoretically determined, depends therefore upon the accuracy with which the value of $\frac{a}{a'}$ has previously been obtained, and the accuracy of these coefficients depends singly and alone upon this, inasmuch as all the other quantities which are used in their calculation are known beforehand with more than sufficient accuracy.

From p. 177 of my *Darlegung der theoretischen Berechnung der in den Mondtafeln angewandten Störungen*, it appears that I have assumed in that calculation,

$$\log \frac{a}{a'} = 7\cdot4034757,$$

* I believe that this is the first indication of the necessity of increasing the Sun's Parallax.—[G. B. A.]

and have obtained this value, by employing the hitherto-assumed Mass* of the Earth $\left[\frac{1}{354936}\right]$. The perturbations assigned in the Lunar Tables, from argument $(g' + \omega - \omega')$ to $(g - 3g' + \omega - 3\omega')$ have been calculated with attention to everything that comes into consideration, and with the above value of $\frac{a}{a'}$. These perturbations do not arise solely from the direct attraction of the Sun, but also from the effect (with changed sign) of the perturbations caused by the Moon in the motion of the Earth (or Sun); and this latter part is taken into account by multiplying the former part by the factor $\frac{M - m}{M + m}$ (M = Earth's mass, m = Moon's mass). The proof† of this theorem I have given in the *Darlegung &c.*, p. 121, &c., because Poisson has raised doubts concerning its accuracy, which appear from this to be unfounded. In p. 177 this factor is denoted by λ, and its numerical value is incorporated with the other factors, there called μ $\left[\text{or } \frac{a}{a'}\right]$ and $u_{,}^2$.‡ In this way has the whole of the following calculation been executed, as will be seen in detail in the second treatise, the printing of which, as already mentioned, will soon begin.

From p. 16 of the Lunar Tables there resulted, by comparison with observations, the factor

$$= 1{\cdot}03973,$$

by which it was necessary to multiply the perturbations named, in order to satisfy the observations; and in the Tables themselves, these perturbations also have been multiplied by that factor. Now, if we multiply the above value of $\frac{a}{a'}$ by this factor, we obtain

$$\log \frac{a}{a'} = 7{\cdot}4187223,$$

the value of this proportion obtained by me from the Lunar Theory, after attention to all the circumstances which come into consideration.

The mean Solar Parallax would be obtained at once from this value, by multiplying it by the value of the Lunar Parallax,

* That is, the proportion of the Earth's Mass to the Sun's Mass. As referred to mechanical units of time and space, the Earth's Mass is known with very great accuracy; and it is really the Sun's Mass, as referred to those units, which is altered by the present investigation. But in the Planetary Theory, it is convenient to assume Sun's Mass = 1; and the language of the text is accommodated to this assumption.—[G. B. A.]

† The proof is rendered extremely simple by adopting as origin of co-ordinates the centre of gravity of the Earth and Moon.—[G. B. A.]

‡ $u_{,}^2 = \frac{\text{Sun}}{\text{Sun} - \text{Earth}} \times \left(\frac{\text{Earth's Mean Motion}}{\text{Moon's Mean Motion}}\right)^2 \times \frac{1}{\sin 1''}$.—[G. B. A.]

corresponding to the radius vector of the Moon $r = a$; but the developments may also be so arranged that the Lunar Parallax does not appear in them at all, and this method, which is more evident and direct than the other, shall be employed here.

The equation

$$\frac{a}{a'} = \sqrt{u^2 \cdot \frac{M + m}{M + m'}},*$$

which has served, by substitution of the hitherto assumed value of m' ($m' =$ Sun's mass) to calculate the above given provisional value of $\frac{a}{a'}$, may now serve, by substitution of the corrected value of $\frac{a}{a'}$, to obtain the corresponding value of m'. It gives finally

$$\frac{m'}{M} = \frac{u^2\left(1 + \frac{m}{M}\right)}{\left(\frac{a}{a'}\right)^3} - 1.$$

We have $\log u = 8{\cdot}8775917$; and if we put $m = \frac{M}{80}$, we obtain

$$m' = 319455\,M$$

for the Sun's mass, or

$$M = \frac{m'}{319455}$$

for the Earth's mass,

as deduced from the Lunar Theory. This value is considerably different from that hitherto assumed.

Let now P be the length of the simple pendulum, which would perform an oscillation in t seconds of mean time, upon the parallel where the sine of the geocentric latitude is $= \sqrt{\frac{1}{3}}$, if the centrifugal force had no existence; D the radius of the Earth at that parallel; and π the proportion of the circumference of a circle to its diameter; then it is shown by Dynamics that

$$t = \pi\sqrt{\frac{P}{k^2 \cdot \frac{M}{D^2}}};\dagger$$

and hence follows

$$k^2 M = \frac{\pi^2 P D^2}{t^2} = \frac{4\pi^2 P D^3}{4 t^2 D}.$$

* $u = \frac{\text{Earth's Mean Motion}}{\text{Moon's Mean Motion}}$.—[G. B. A.]

† k^2 M, in German Physical Astronomy, corresponds to what, in English works, is usually called "the Mass," as represented by dynamical units.—[G. B. A.]

Now, if we call T′ the period of the Earth's sidereal revolution round the Sun, and n' its mean motion, we have, whatever be the fundamental units of n' and T′, $2\pi = n'T'$; moreover, if we call p' the mean horizontal parallax of the Sun at the same parallel, then $D = a' \sin p'$; and thus the foregoing equation is transformed into

$$k^2 M = \frac{a'^3 n'^2 T'^2 P \sin^3 p'}{4 t^2 D}.$$

But Keppler's third law gives

$$a'^3 n'^2 = k^2 (M + m'),$$

so that

$$\sin p' = \sqrt[3]{\frac{4 t^2 D M}{T'^2 P (M + m')}}.$$

This expression becomes homogeneous by expressing T′ in seconds of mean time, and expressing P and D in the same units of length. The Solar Tables give

$$T' = 31558149^s;$$

and following Bessel, I have assumed the logarithm of the equatoreal semidiameter of the Earth expressed in metres. = 6·8046271. Since, moreover, the logarithm, which must be added to this, in order to obtain the log. semidiameter to the above-mentioned parallel, is 9·9995166,

$$D = 6370063^m.$$

For P I have always assumed the value

$$P = 0^m . 992666 \frac{434 \cdot 86}{433 \cdot 86} * \text{ for } t = 1^s.$$

(See *Darlegung*, &c., p. 415.) Now, by substituting these values in the above equation, p' becomes 8·9060 for the parallel in question; and by reducing this to the equator, by the factor given above, the mean equatoreal horizontal Parallax of the Sun becomes = 8″·9159, agreeing very nearly with the values which have been recently obtained in other ways.

* The proportion of the centrifugal force at the equator to gravity = ·003467; the effect in the vertical at latitude l = ·003467 × $\cos^2 l$. When $\sin^2 l = \frac{1}{3}$, $\cos^2 l = \frac{2}{3}$, and this effect = $\frac{1}{434 \cdot 86}$ nearly.—[G. B. A.]

On the origin of the apparent luminous band which, in partial eclipses of the Sun, has been seen to surround the visible portion of the Moon's limb. By George Biddell Airy, Astronomer Royal.

Professor Challis, in his paper "On the Indications by Phenomena of Atmospheres to the Sun, Moon, and Planets," published in the *Monthly Notice* for 1863, June 12, has remarked, "if there be a lunar atmosphere, no effect would be at all likely to be discernible, except a slight condensation of solar light bordering the part of the Moon's periphery which crosses the Sun's disk." And Professor Challis then adverts to Professor Stephen Alexander's observation of such a brighter band bordering the edge of the Moon in a solar eclipse, and to the depiction of it in daguerréotypes of the eclipsed Sun.

I infer from this that Professor Challis assumes that, if a part of the Sun's disk is viewed through a refracting medium which produces an effect analogous to that of a convex lens, that part of the disk will appear brighter than the rest. It is my principal object in this paper to show, by optical investigation, that no refraction can cause a change in the apparent brightness of the surface viewed. Having regard to the astronomical importance of the subject, I think it necessary to go into the details of the investigation. I am not aware that any optical writer has examined into this question, and I present my solution nearly in the form in which I worked it out for my own satisfaction many years ago. Probably an accomplished mathematician who is familiar with the treatment of waves can exhibit the demonstration in a less cumbrous form.

My process will be this:—Assuming that a telescope of given aperture T is employed to look through the refracting medium at the Sun's disk (or any other surface), I shall ascertain the value of the pencil-angle t, of the pencil of rays emanating from any point under view, which will produce a pencil that will exactly fill the object-glass T. This is the measure of the light proceeding from any one point of the Sun. Then assuming that the field of view (or as much of it as we think fit to consider) is expressed by the visual angle σ at the object-glass, I shall ascertain the linear breadth S of so much of the Sun's disk as is seen at once in that definite field of view: this is the measure of the number of luminous points of the Sun under view. The product t S is the measure of the whole quantity of light which fills the definite field of view. And this, it is to be shown, is the same as if there were no refraction.

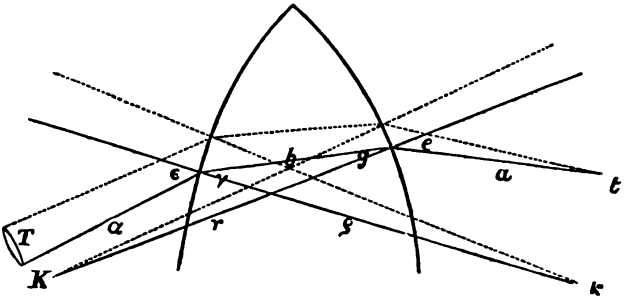


Fig. 1.

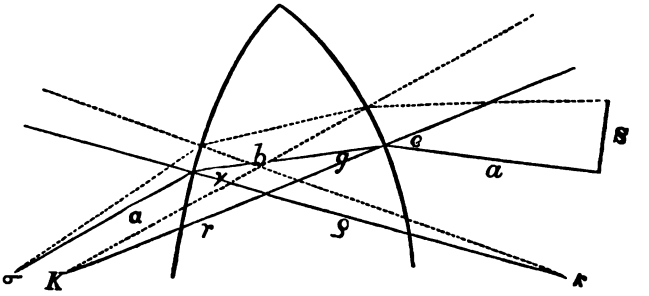


Fig. 2.

In the diagrams above, let the curved figure represent the outline of a refracting medium (the same in the two diagrams): in the first, let a pencil of light, diverging from t a point on the Sun's disk, ultimately fill the whole object-glass T; and in the second, let the axes of pencils be carried backward from the point σ of the object-glass, diverging at a definite angle σ corresponding to the definite field of view under consideration, and, after undergoing the refractions, let them include the breadth S on the Sun's disk. K and κ are the centres of curvature, and r and ρ the radii of curvature, for the points of the refracting surface where the refractions take place. e is the first angle of incidence for one ray, and g the corresponding angle of internal refraction; γ the second angle of (internal) incidence, and ϵ its angle of refraction. For the neighbouring rays of pencil (in fig. 1), or neighbouring axes of pencils (in fig. 2), the symbol δ is prefixed to these letters. a, b, α are the lengths of the three portions of the broken ray.

First, let us trace the circumstances of the pencil in fig. 1.

Its transverse breadth, where it falls on the first surface, is $a\,t$.

The breadth occupied on the first surface is $\frac{a\,t}{\cos e}$.

The inclination of the two radii of curvature for the two limiting rays is $\frac{a\,t}{r\,.\cos e}$.

δe, the difference of the angles of incidence $= t + \frac{a\,t}{r\,.\cos e}$.

δg, the difference of the angles of refraction =

$$\frac{d\,g}{d\,e}\,.\,t\,.\left(1 + \frac{a}{r\,.\cos e}\right).$$

The inclination of the two limiting rays after the first refraction $= \delta g$ — inclination of radii of curvature =

$$t\left(\frac{d\,g}{d\,e} + \frac{a}{r\cos e}\,.\,\frac{d\,g}{d\,e} - \frac{a}{r\cos e}\right).$$

The transverse breadth of the pencil after the first refraction $= \frac{a\,t\,.\cos g}{\cos e}$.

After describing the path b, the transverse breadth of the pencil = breadth after first refraction + b × inclination of limiting rays =

$$t \times \left\{\frac{a\,.\cos g}{\cos e} + b\,.\,\frac{d\,g}{d\,e} + \frac{a\,b}{r\cos e}\,.\,\frac{d\,g}{d\,e} - \frac{a\,b}{r\cos e}\right\}.$$

The breadth occupied on the second surface is

$$t \times \left\{\frac{a\,.\cos g}{\cos\gamma\,.\cos e} + \frac{b}{\cos\gamma}\,.\,\frac{d\,g}{d\,e} + \frac{a\,b}{r\,.\cos\gamma\,.\cos e}\,.\,\frac{d\,g}{d\,e} - \frac{a\,b}{r\,.\cos\gamma\,.\cos e}\right\}.$$

The inclination of the two radii of curvature for the limiting rays is

$$t \times \left\{\frac{a\,.\cos g}{\varrho\,.\cos\gamma\,.\cos e} + \frac{b}{\varrho\,.\cos\gamma}\,.\,\frac{d\,g}{d\,e} + \frac{a\,b}{\varrho\cos\gamma\,.\,r\cos e}\,.\,\frac{d\,g}{d\,e} - \frac{a\,b}{\varrho\cos\gamma\,.\,r\cos e}\right\}.$$

$\delta\gamma$ = inclination of radii—inclination of limiting rays after the first refraction.

$$\delta\varepsilon = \frac{d\,\varepsilon}{d\,\gamma} \times \left\{\begin{array}{l}\text{inclination of radii — inclination of limiting}\\ \quad\text{rays after the first refraction.}\end{array}\right\}$$

The inclination of the two limiting rays after the second refraction = inclination of radii $-\delta\varepsilon = \left(1 - \frac{d\,\varepsilon}{d\,\gamma}\right)$ × inclination

of radii $+ \frac{d\,\epsilon}{d\,\gamma} \times$ inclination of limiting rays after the first refraction =

$$t \times \left(1 - \frac{d\,\epsilon}{d\,\gamma}\right) \times \left\{\frac{a \cos g}{\varrho \cos \gamma \,.\, \cos e} + \frac{b}{\varrho \cos \gamma} \,.\, \frac{d\,g}{d\,e} + \frac{a\,b}{\varrho \cos \gamma \,.\, r \cos e} \,.\, \frac{d\,g}{d\,e} - \frac{a\,b}{\varrho \cos \gamma \,.\, r \cos e}\right\}$$

$$+ t \times \left\{\frac{d\,\epsilon}{d\,\gamma} \,.\, \frac{d\,g}{d\,e} + \frac{a}{r \cos e} \,.\, \frac{d\,\epsilon}{d\,\gamma} \,.\, \frac{d\,g}{d\,e} - \frac{a}{r \cos e} \,.\, \frac{d\,\epsilon}{d\,\gamma}\right\}.$$

The transverse breadth of the pencil after the second refraction = breadth on second surface $\times \cos \epsilon =$

$$t \times \left\{\frac{a \,.\, \cos \epsilon \,.\, \cos g}{\cos \gamma \,.\, \cos e} + \frac{b \cos \epsilon}{\cos \gamma} \,.\, \frac{d\,g}{d\,e} + \frac{a\,b \,.\, \cos \epsilon}{r \,.\, \cos \gamma \,.\, \cos e} \,.\, \frac{d\,g}{d\,e} - \frac{a\,b \cos \epsilon}{r \,.\, \cos \gamma \,.\, \cos e}\right\}.$$

And the ultimate transverse breadth of the pencil (which hereafter is to be made = T) is = breadth after second refraction $+ \alpha \times$ inclination of limiting rays after second refraction =

$$t \times \left\{\frac{a \,.\, \cos \epsilon \,.\, \cos g}{\cos \gamma \,.\, \cos e} + \frac{b \cos \epsilon}{\cos \gamma} \,.\, \frac{d\,g}{d\,e} + \frac{a\,b \,.\, \cos \epsilon}{r \,.\, \cos \gamma \,.\, \cos e} \,.\, \frac{d\,g}{d\,e} - \frac{a\,b \cos \epsilon}{r \,.\, \cos \gamma \,.\, \cos e}\right\}$$

$$+ t \times \left(1 - \frac{d\,\epsilon}{d\,\gamma}\right) \times \left\{\frac{\alpha\,a \cos g}{\varrho \cos \gamma \,.\, \cos e} + \frac{\alpha\,b}{\varrho \cos \gamma} \,.\, \frac{d\,g}{d\,e} + \frac{\alpha\,a\,b}{\varrho \cos \gamma \,.\, r \cos e} \,.\, \frac{d\,g}{d\,e} - \frac{\alpha\,a\,b}{\varrho \cos \gamma \,.\, r \cos e}\right\}$$

$$+ t \times \left\{\alpha \,.\, \frac{d\,\epsilon}{d\,\gamma} \,.\, \frac{d\,g}{d\,e} + \frac{\alpha\,a}{r \cos e} \,.\, \frac{d\,\epsilon}{d\,\gamma} \,.\, \frac{d\,g}{d\,e} - \frac{\alpha\,a}{r \cos e} \,.\, \frac{d\,\epsilon}{d\,\gamma}\right\}.$$

Let the entire factor of t in the last expression be called V. Then we must have $t \times \mathrm{V} = \mathrm{T}$, or $t = \frac{\mathrm{T}}{\mathrm{V}}$.

Secondly, let us trace the circumstances of the diverging axes of pencils in fig. 2.

The investigation is in every respect similar, and the form of the result is precisely similar, with due changes of symbols. We find $\sigma \times \mathrm{W} = \mathrm{S}$, where W may be formed from V by putting α for a and a for α, ϵ for e and e for ϵ, γ for g and g for γ, ϱ for r and r for ϱ; b occupies the same place in both formulæ.

And, this being done, the quantity of light which comes to

the eye in a definite portion of the field of view, or t S, will $= \frac{T}{V} \times \sigma \times W = T\sigma \times \frac{W}{V}$. As T and σ are given, by the construction of the telescope, the quantity of light which comes to the eye in a definite portion of the field of view is proportional to $\frac{W}{V}$.

It does not appear possible to assign any value to this fraction until we have assigned the proper forms to the differential coefficients which occur in the expressions. To do this, let $\sin e = n \,.\, \sin g$ (according to the received law of refraction); then $\cos e = n \,.\, \cos g \,.\, \frac{dg}{de}$, or $\frac{dg}{de} = \frac{1}{n} \,.\, \frac{\cos e}{\cos g}$. Similarly $\frac{d\iota}{d\gamma} = n \,.\, \frac{\cos \gamma}{\cos \iota}$. Substituting these, V becomes =

$$\frac{a \,.\, \cos \iota \,.\, \cos g}{\cos \gamma \,.\, \cos e} + \frac{b \,.\, \cos \iota}{\cos \gamma} \,.\, \frac{1}{n} \,.\, \frac{\cos e}{\cos g} + \frac{a\, b \cos \iota}{r \,.\, \cos \gamma \,.\, \cos e} \,.\, \frac{1}{n} \,.\, \frac{\cos e}{\cos g} - \frac{a\, b \,.\, \cos \iota}{r \,.\, \cos \gamma \,.\, \cos e}$$

$$+ \left(1 - n \frac{\cos \gamma}{\cos \iota}\right) \times \left\{ \frac{\alpha\, a \cos g}{\varrho \cos \gamma \,.\, \cos e} + \frac{\alpha\, b}{\varrho \cos \gamma} \,.\, \frac{1}{n} \,.\, \frac{\cos e}{\cos g} + \right.$$

$$\left. \frac{\alpha\, a\, b}{\varrho \cos \gamma \,.\, r \cos e} \,.\, \frac{1}{n} \,.\, \frac{\cos e}{\cos g} - \frac{\alpha\, a\, b}{\varrho \cos \gamma \,.\, r \cos e} \right\}$$

$$+ \frac{\alpha \cos \gamma \,.\, \cos e}{\cos \iota \,.\, \cos g} + \frac{\alpha\, a \,.\, \cos \gamma}{r \,.\, \cos \iota \,.\, \cos g} - n \frac{\alpha\, a \,.\, \cos \gamma}{r \cos e \,.\, \cos \iota};$$

which will be more easily understood if we divide it into three separate classes of terms, and arrange the individual terms as below:—

Terms of V independent of n:

$$\frac{\alpha\, a \,.\, \cos g}{\varrho \cos \gamma \,.\, \cos e} + \frac{a\, \alpha \cos \gamma}{r \cos g \,.\, \cos \iota}$$

$$- \frac{\alpha\, a \,.\, b}{\varrho \cos \gamma \,.\, r \cos e} - \frac{a\, \alpha \,.\, b}{r \cos g \,.\, \varrho \cos \iota}$$

$$- \frac{\alpha \cos e \,.\, b}{\varrho \cos \iota \,.\, \cos g} - \frac{a \cos \iota \,.\, b}{r \cos e \,.\, \cos \gamma}$$

$$+ \frac{\alpha \cos \gamma \,.\, \cos e}{\cos \iota \,.\, \cos g} + \frac{a \cos g \,.\, \cos \iota}{\cos e \,.\, \cos \gamma}.$$

Terms of V multiplying n:

$$\frac{\alpha a \,.\, b}{\varrho \cos \iota \,.\, r \cos e}$$

$$-\frac{\alpha a \,.\, \cos g}{\varrho \cos \iota \,.\, \cos e} - \frac{\alpha a \,.\, \cos \gamma}{r \cos e \,.\, \cos \iota}.$$

Terms of V multiplying $\frac{1}{n}$:

$$\frac{\alpha \cos e \,.\, b}{\varrho \cos \gamma \,.\, \cos g} + \frac{a \cos \iota \,.\, b}{r \cos g \,.\, \cos \gamma}$$

$$+ \frac{\alpha a \,.\, b}{\varrho \cos \gamma \,.\, r \cos g}$$

$$+ \frac{\cos \iota \,.\, \cos e \,.\, b}{\cos \gamma \,.\, \cos g}.$$

Now, as has been said, we are to form W from V by interchanging a and α, e and ι, g and γ, r and ϱ. But on proceeding to do this, we shall see immediately that each line of the expression, as last arranged, is strictly symmetrical with regard to these quantities, and therefore the interchange produces no alteration whatever in it; and, consequently, $W = V$; and the light which comes into a definite portion of the field of view of the telescope $= T\sigma \times \frac{W}{V} = T\sigma$, or is exactly the same as if no refracting medium were interposed.

And therefore refraction by a lunar atmosphere cannot explain the more luminous band which appears to surround the Moon's limb where it crosses the Sun's disk.

I have no difficulty in explaining to myself the origin of the luminous band in question. It is strictly an ocular nervous phenomenon, not properly subjective, but sensational—a mere effect of contrast. I have seen it so frequently under circumstances very different from these, that I cannot have the smallest doubt on the matter.

A person who stands before a moderately illuminated whitish wall, and swings a piece of black cloth upwards between his eye and the wall, will almost always see the edge of the black cloth preceded by a broad luminous band. I should once have said that such a band is always seen, but I find that I do not see it so conspicuously now as formerly. The continuous decay of the nervous irritability of the retina is, I believe, an ordinary physiological fact. When I used to

see it best, its angular breadth, as measured by the visual angle at the eye, was not less than two degrees. This I believe to be the sole and sufficient explanation of the phenomenon in question, both in the eye-view of the material Moon and in the eye-view of the photographs of the Moon. In the latter case, as I understand, the explanation is substantiated by Mr. De La Rue's experiments.

Royal Observatory, Greenwich,
1863, *Sept.* 5.

On the Extension of Lunar Nomenclature. By W. R. Birt, Esq.

During the course of my lunar inquiries, I have met with several craters, some of very considerable size, and other portions of the Moon's surface, that have been destitute of names, and in a few instances have not even been delineated on our maps. Taking the large map of Beer and Mädler, as one of our principal lunar authorities, I have, in conjunction with Dr. Lee — who suggested to me the importance of distinctly designating such portions of the Moon's surface, as contributing greatly to the advance of Selenography — named those which up to this time appeared sufficiently prominent to merit the distinction. Some of these names are already before the public, and a few may be found for the first time in the following list. As it is not improbable that these names may be retained by astronomers, both Dr. Lee and I have thought it advisable to make this communication to the Society, that they may be permanently recorded in its Archives.

It will be desirable to record, with the names of these lunar spots, their selenographical Longitudes and Latitudes, and also to carry on the numbering adopted by the Rev. T. W. Webb, in his little, but useful work, *Celestial Objects for Common Telescopes.*

Recently-named Lunar Craters, &c.

I. *North-West Quadrant.*

		Lat.	Long.
405	The Coxwell Mountains	10° N. to 15° N.	45° W.
406	Mount Glaisher	15° N.	46° W.
407	Chevallier	45° N.	50° W.
408	Moigno	63° N.	25° W.
409	Peters	68° N.	25° W.

II. *North-East Quadrant.*

410	The Teneriffe Mountains	41° N. to 48° N.	3° E. to 18° E.
411	Piazzi Smyth	42° N.	3° 30′ E.
412*	Herschel II.	58° N. to 64° N.	30° E. to 40° E.
413*	Robinson	58° N. to 63° N.	40° E. to 50° E.
414*	South	55° N. to 58° N.	45° E. to 53° E.
415	Babbage	57° N. to 62° N.	52° E. to 60° E.

III. *South-East Quadrant.*

416	The Percy Mountains	17° S. to 25° S.	41° E. to 53° E.
417*	Rosse	53° S. to 60° S.	48° E. to 55° E.

IV. *South-West Quadrant.*

418	J. Franklin	12° 30′ S. to 18° 30′ S.	47° W. to 52° W.
419	Crozier	15° S.	50° W.
420	M'Clure	15° S.	48° W.
421		13° S.	49° W.
422	Wrottesley	23° S.	56° W.
423	Phillips	24° S. to 27° S.	73° W. to 80° W.
424*	The Mare Smythii	5° 30′ N. to 9° S.	80° W. to 95° W.

The authority for the above co-ordinates is Beer and Mädler's map; they, however, require a careful redetermination.

Hartwell Observatory, Nov. 12, 1863.

On certain Depressions on the Moon's Western Limb.
By the Rev. Henry Cooper Key.

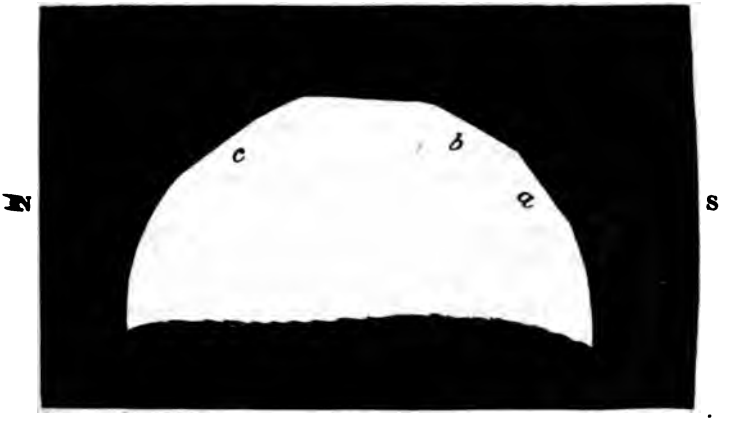
Although it has long been known that many depressions from the general level of the Moon's surface are to be found upon her disk, and that some of these depressions are of large extent, probably much beyond anything of the same kind existing on the Earth, if we except the beds of our oceans and seas, still this singular circumstance has no doubt attracted the attention of many observers that the depressions in question seem to be confined to the interior portion of the Moon's disk, and that none are visible on the limbs during any part of her libration. I speak of depressions of large area—not of comparatively small gullies, lying between elevated ranges, which

* Those marked * are unrepresented in Beer and Mädler's large map, but some very imperfect indications of them exist.

are constantly to be seen projected on the limb, but of vast tracts, the general level of which lies very considerably beneath the mean level of the Moon's surface.

I have lately had the good fortune to witness the projection on the limb of the Moon of several of these tracts of large area, the largest of which extended across more than 35° of latitude, and its deepest depression below the general level of the Moon's surface could hardly be less than twenty-five miles. These amounts are merely rough estimates, but will probably be found to be within rather than beyond the truth.

Having mounted my new 12-inch glass speculum, I had for some time past been making experiments with it, in the unsilvered state, upon celestial objects, with the view of ascertaining how far a diminution of light and consequently of the evils of irradiation, combined with a large aperture, might be an advantage in particular cases. For this purpose, on the 20th of September, at about 6 P.M., before the Sun had set, I turned the telescope upon the Moon, then a few hours past her first quarter. I had no sooner focussed the telescope (power 120), than I was astounded at observing that the limb of the Moon was entirely *out of shape;* that it was, in fact, irregularly polygonal, as if several large segments had been cut off the spherical limb — not the terminator. Of course I at once changed the eye-piece (using powers of 85 and 203), and looked to see whether the instrument was out of adjustment, but the cause of this extraordinary appearance was clearly not in the telescope.



The accompanying diagram,* drawn from recollection the following day, is, I am sorry to say, the only representation I am able to exhibit; and this merely indicates, in a rough man-

* The letters *a*, *b*, and *c* stand opposite to the middle of the depressions respectively.

ner, the position of these large areas of depression. It did not occur to me, at this time, that libration and bad weather combined might prevent my making drawings and measurements another night: I looked upon the feature as a permanent one, the details of which I could secure at any time.

From the absence of all irradiation, the limb of the Moon was far more beautifully defined than I ever saw it before; and one or two mountains stood up in wonderfully clear relief on the north-western limb, in the neighbourhood, I think, of *Strabo*.

The same evening, at about 8 P.M., I verified these appearances with an excellent 8-inch (silvered glass speculum) Newtonian. They were evident enough, but not nearly so much so as in the larger instrument, two hours before: indeed I felt satisfied that I should not have noticed them had they not been pointed out in so glaring a manner by the larger unsilvered speculum.

Having written to my friend the Rev. T. W. Webb, of Hardwick, upon the subject, on the following day, Sept. 21st, enclosing him a diagram, I received a letter from him on the 25th Sept., in which he says: "I had not the least recollection, from your diagram, where to look for the flattenings, and, to say the truth, was not prepossessed in their favour, thinking they might have arisen from optical imperfection; or rather, as you used two telescopes, from irregular atmospheric refraction. At any rate, I was perfectly free from the expectation of seeing them; but, with a power of 64, there they were *at once*." And in a later letter, dated Sept. 30th, he says: "There is no doubt whatever about your depressions. I tried them with the micrometer, and the immediate presence of the wire proved beyond the possibility of doubt that they were actually flat; one was a little concave. . . . . The flattenings must be very extensive, as I found them unaffected by a considerable amount of libration."

In the meantime, owing to the painters being at work, I was unable to use my large instrument until Friday Sept. 25th. I then made preparation for taking correct alignments, &c., so as to fix the positions of these depressions with something like accuracy; but to my great disappointment, on turning the instrument upon the Moon, I found that, with the exception of a comparatively faint trace, they had entirely disappeared; the Moon's limb presenting its usual uniform curve. Nor during the present lunation have I been able (from the 15th to the 23d of October) to detect any but the slightest trace of them. I ought to state, however, that during the interval the speculum had been silvered.

The appearance of these flat surfaces was so extraordinary (they are hardly at all exaggerated in the diagram) that, had not my own observations been fully confirmed by those of Mr. Webb, I should hardly have ventured to make public mention

of them. But as there can now be no doubt whatever about their existence, I trust some close observer of the Moon will search for them, and before long find them again, and fix their positions accurately.

From a still later communication from Mr. Webb I am led to think, that, although he undoubtedly saw these flattenings, and was able to fix the position in latitude of some of them pretty clearly, yet that he did not see anything like the same extraordinary effect that I witnessed at 6 P.M. on Sept. 20th, when the Moon's limb was altered in shape in so surprising a manner. The depressions which he observed, and which required close scrutiny from the first, he was able to keep in view for some days after, and even remarked them during the present lunation, up to Oct. 22d; whereas the astonishing appearance of the Moon's limb which I noticed at 6 P.M. on the 20th of Sept., was so completely gone on Friday the 25th Sept. that I turned away disappointed, and did not stop to examine the faint traces which were still remaining.

My conclusion therefore is,—in the first place,—that the maximum of visibility of these depressions lasts but a short time, probably not more than forty-eight hours at the most, or from three to four days after the greatest south-east libration; that in the absence of some means for annihilating the evils of irradiation at the Moon's limb, the full extent of them is not readily to be seen, and would not easily be credited; that the chances of their being noticed with ordinary instruments, by any one who was not previously aware of their existence, are very small indeed; and that it is entirely to these circumstances that their present accidental discovery is to be attributed.

The effect of irradiation in obliterating the true outline of an object, and unduly enlarging its diameter or extent, is shown in a striking manner by the fact that a piece of platinum wire of the thickness of a very fine hair, when intensely heated by the voltaic current, so as to become extremely luminous, immediately appears to be as thick as coarse bell-wire. Another obvious instance, and very *àpropos*, is to be seen in the young Moon, when the unillumined portion of her disk is visible; owing to irradiation, that part which is illumined by the Sun's rays appears to be a portion of a considerably larger circle than that which bounds the rest of the disk. It ought not then to surprise us, to find that the strong irradiation at the Moon's limb affects her outline to a very serious extent, even sufficiently to obliterate inequalities subtending an angle of many seconds.

To Sir John Herschel belongs the credit of first suggesting the use of plain glass specula in the Newtonian telescope for observing the Sun; and I am convinced from my own experience already that additional information will be obtained respecting the nature of the Sun's surface by this means. But their use should not by any means be confined to the Sun;

wherever the brilliance of an object is sufficient, I strongly advocate the use of the plain in addition to the silvered speculum; for instance, upon large fixed stars, and the planets *Mars*, *Jupiter*, *Saturn*, and especially *Venus*. The delicate pencilling on the surface of the planet *Jupiter* is brought out by this means with marvellous effect; the diminished light of course increases the effect of the darker markings; while at the same time the eye, being less stimulated by the brighter portions, is able to expand with comfort, and to discern stipplings and shade of exceeding faintness, the existence of which could not even be suspected in an ordinary instrument of equal aperture. The effect on the planet *Saturn* is quite stereoscopic; the light of the ball being so strongly contrasted with that of the rings, that the body of the planet seems, as it were, isolated and suspended within the rings, in a far more marked manner than in an ordinary instrument. But more especially should advantage be taken of the total absence of irradiation, obtainable by this means, to observe the occultations both of fixed stars and planets by the Moon, as being the most likely method of obtaining more trustworthy evidence respecting her atmosphere.

Stretton Rectory, Hereford,
Nov. 3, 1863.

Observations of Venus, made near her Inferior Conjunction.
By Capt. W. Noble.

On Monday, the 28th of September, I examined the planet, when she was from $6^h\ 36^m$ to $6^h\ 20^m$ from her inferior conjunction. The apparent tenuity of her crescent was very striking; the cusps were perceptibly fainter than the middle of the limb. I did not attempt to measure the width of the illuminated portion, but did obtain a few micrometrical measurements of the distance between her cusps, and the mean of these gave it as $61''\cdot3068$. The diameter of the planet given in the *Nautical Almanac* for that day being $59''$, I did not look for the dark body of the planet; but from what I subsequently observed on Friday, the 9th of October, have no doubt that proper precautions would have enabled me to see the whole of the limb at the time of her inferior conjunction.

Forest Lodge, Maresfield,
12th Nov., 1863.

Observations of Venus at the Inferior Conjunction.
By C. Leeson Prince, Esq.

Although *Venus* was not favourably situated for observation at the time of inferior conjunction last month, yet, as I took some interest in watching the planet almost daily from the 23d to the 30th, both inclusive, the following remarks may perhaps be interesting to those who have been similarly engaged:—

"Sept. 23d. Observing *Venus* to-day just about the time of meridian passage, I *thought* I saw the whole disk slightly illuminated; but as the atmosphere was in a very disturbed state, I could not be quite certain.

"25th. Upon looking at the planet again to-day, I am satisfied that I not only saw the dark body, but also a phosphorescent flitting of light around the edge of the entire disk. Notwithstanding the stormy weather, the very narrow illuminated portion was well seen.

"26th. The dark body of the planet seen again to-day, but not so distinctly as yesterday.

"27th. Planet not seen well to-day, the atmosphere being very tremulous. Dark body of the planet not seen, but there were glimpses of light around the edge of the disk, as observed on 25th.

"28th. A very stormy morning, with driving rain. No opportunity occurred of looking at the planet until 2 P.M., when I observed that there had been no appreciable difference in the breadth of the illuminated portion since 25th; but the horns of the crescent had lessened, and did not extend to a semidiameter of the planet. The dark body was faintly seen. Diameter of planet, measured with Jones's double-image micrometer, 58″·7.

"30th. As the planet passed the meridian to-day, I again observed the phosphorescent flitting of light around the *edge* of the disk, which I had observed both on 25th and 27th; but I could not see the dark body of the planet which I noticed on 23d, 25th, 26th, and 28th."

From this date stormy wet weather prevented any further observations till Oct. 6, at which time the dark body of the planet was no longer visible, and the horns of the crescent were still less than a semidiameter of the planet.

P.S. I had almost forgotten to mention that I always saw the above phenomenon best by using a power of 150 with Dawes' solar eye-piece and ⅛-inch diaphragm.

Uckfield, Oct. 17th, 1863.

On the Occultation of κ Cancri by the Moon on the 26th April, 1863. By C. Leeson Prince, Esq.

My attention having lately been called to the letter of the Rev. W. R. Dawes in the *Monthly Notices*, vol. xxiii. p. 251, respecting the occultation of κ *Cancri* by the Moon on April 26th, I am induced to send the copy of a memorandum which I have found in my Observatory-book respecting that phenomenon, inasmuch as it confirms Mr. Copeland's observation, and points to the advantages of observing these phenomena with telescopes of considerable aperture.

"April 26th. κ *Cancri* occulted by the Moon at $12^h 3^m 32^s.5$. The disappearance was *not* instantaneous by fully half a second."

About an hour previously I had watched the disappearance of *Jupiter's* second satellite, when I noticed that the satellite appeared projected upon the planet for at least thirty seconds before its disappearance. At the time I attributed both phenomena to some peculiar state of our atmosphere. The day had been cloudless; barometer (reduced) at 9 A.M., 30·358; wind N.W. The telescope employed was of 6·8 inches aperture, and 12 feet focal length; power 150.

Uckfield, Oct. 17th, 1863.

In reference to the same Occultation, the Astronomer Royal has communicated an observer's note, which should have been sent with the observations, as printed *Monthly Notices*, t. xxiii. p. 248.

The observer's (Mr. Creswick's) note runs as follows:—

"The star did not seem to disappear with the suddenness I have generally noticed."

Mr. Creswick's impression is, that there was a loss of light at about one-tenth of a second before the final disappearance. Mr. Creswick is certain that, at Greenwich, there was no great loss of light five-tenths of a second before the final disappearance.

Observations of the Sun at Rising to Determine the Refraction; and of Solar Spots. By Sir Andrew Lang.

I take leave to forward a few more observations on the rising of the Sun, at my position, for deducing the Refraction. I could have wished to have made many more, but long intervals will occur before you can obtain a sight of the Sun rising from

the horizon of the sea, from beds of cirri prevailing at the horizon, rendering it impossible.

Professor Drew, in his valuable work on Meteorology, says the cumulus is formed only in the daytime. This is certainly not the case in the West Indies, where the cumulus prevails as much at night as during the day, forming a continuous stream of cloud, floating at a height of about two thousand feet above the level of the sea.

The *solar tides of the atmosphere* are as regular as clock-work, rising and falling every six hours.

4 to 10 A.M. rising; 10 A.M. to 4 P.M. falling.
4 to 10 P.M. rising; 10 P.M. to 4 A.M. falling.

Any disturbance therein, especially during our hurricane months, is a certain sure warning to be vigilant and suspicious of the weather.

Some observations made at my position, in the Island St. Croix (Lat. 17° 44′ 32″ N., Long. 64° 41′ W. from Greenwich), on the Sun's centre rising from the horizon of the sea, to determine the Refraction.

Place of observation 440 feet above the level of the sea; angle of depression of the sea horizon 20′ 56″.

	Refraction and Parallax.	Barometer.	Therm. Fahr.
	′ ″		°
1860, June 5	32 5·15	29·567	77·4
6	32 20	·602	77
9	32 4·20	·646	77·2
10	32 4	·58	76
24	32 2·4	·57	77
27	32 10·8	·67	78·6
1861, May 25	32 38·6	·36	76
July 19	32 24·7	·61	77
27	32 28·8	·62	79·3
28	32 30	·65	78·5
Aug. 1	32 19	·62	77
1863, May 21	32 10·2	·708	77
June 5	32 12	·610	73·6
9	32 12	·682	78
22	32 4·2	·700	76·5
29	32 1·0	·646	76
July 9	32 12	·660	76
11	32 12	·646	77
Mean of the 18 Observations	32 14	29·631	76·94

More correctly it should be stated that, for my own convenience, the above eighteen observations have been made at my dwelling, 3″ South of my Observatory there, in Lat. 17° 44′ 29″ N., Long. the same, 64° 41′ West. Height of the eye above the level of the sea 420 feet, and angle of depression of horizon of sea 20′ 26″.

Some remarks regarding the Sun, which I examine, I may say, every morning, when at an altitude of 15° to 20°, and note the appearance of any new spot, cluster, or chain.

In 1862 my number of these is 148; in 1861, as already reported, 226; and in 1860, 274.

1862, June 10. A connected chain of spots on disk, about 120,000 English miles in length.

July 23 to Aug. 3. A monster black spot observed, about 35,000 English miles in diameter.

Sept. 24. Two parallel chains of spots, about 180,000 English miles in length each.

Oct. 8, 9. Two beautiful chains of spots, vertical, close to each other, near the centre, about 100,000 English miles in length each.

Nov. 22. A splendid chain of spots, about 140,000 English miles in length.

Dec. 31. A large cluster of spots, No. 148, the last number for the year.

1863, April 1. An immense chain of spots for several days; must be noticed in Europe.

Aug. 24 to Sept. 9. Not a spot on disk; an extraordinary occurrence.

Sept. 9. A small spot, No. 9, and a small chain, No. 91, disappear; dissipated on the 12th.

Sept. 12 to 22. Not a spot on disk.

Sept. 23. A large spot comes on upon Limb, No. 92.

Sept. 29. A great cluster just come on, No. 93.

Oct. 3. No. 93 has now become a connected chain of spots, with a large black spot at each extremity, 90,000 English miles in length.

Oct. 4. No. 99 has disappeared by rotation; No. 94, a small spot, just come on.

St. Croix, West Indies, Oct. 4, 1863.

Elements and Ephemeris of Comet IV. 1863.
By M. Hermann Romberg.

(*Communicated by G. Barclay, Esq.*)

This Comet was discovered on Oct. 9, in R.A. $9^h\ 36^m$, Decl. +29°, by Mr. Bäcker, at Nauen. From two observations taken at Leipsic and one at Vienna, viz.:—

1863.	G.M.T. h m s	Comet's R.A. ° ′	Comet's Decl. ° ′ ″
Oct. 11	13 25 43	146 41 18·3	+30 22 7·3
14	13 35 14	149 1 53·7	31 24 0·9
16	15 3 40	150 43 4·0	+32 8 22·9

I calculated the following parabolic elements, taking into account the small corrections of Parallax and Aberration:—

T 1863, Dec. 27·70863 Greenwich M.T.

π	180° 17′ 53″·4	Apparent Equinox of Oct. 14.5.
☊	104 51 28·8	
i	+ 82 16 29·4	
log q	0·131934	

Direct.

The errors of the mean place being

C − O $\Delta\lambda = -12''\cdot7$ $\Delta\beta + 2''\cdot3$

From these elements I calculated the following Ephemeris, giving the place of the Comet with sufficient exactness:—

0h Gr. 1863.	Comet's R.A. h m s	Comet's Decl. ° ′	Log Δ.	Bright.
Nov. 6	11 30 46	+40 25·7	0·1864	2·35
8	11 42 7	41 15·7	·1783	
10	11 54 3	42 4·4	·1705	
12	12 6 36	42 51·4	·1631	
14	12 19 45	43 35·7	·1562	2·89
16	12 33 31	44 16·9	·1499	
18	12 47 51	44 54·3	·1441	
20	13 2 44	45 27·1	·1390	
22	13 18 6	45 54·7	·1344	3·39
24	13 33 53	46 16·3	·1306	
26	13 49 59	46 31·7	·1276	
28	14 6 17	46 40·1	·1254	
30	14 22 41	46 41·8	·1239	3·72
Dec. 2	14 39 1	46 36·2	·1232	
4	14 55 15	46 23·9	·1233	
6	15 11 11	46 4·8	·1242	
8	15 26 45	45 39·7	·1258	3·90
10	15 41 50	45 8·7	·1281	
12	15 56 25	44 32·7	·1310	
14	16 10 23	43 52·6	·1345	
16	16 23 46	+43 8·6	0·1387	3·72

The brightness of the Comet on Oct. 9 taken $=1\cdot00$. The Comet will probably not become visible by the naked eye.

Observatory of G. Barclay, Esq.
Leyton, 1863, *Nov.* 8.

New Minor Planet (79), *discovered by James Watson, Director of the Observatory of Ann Arbor.*

The original observations were

		R.A.	Decl.
	h m	h m s	° ′
1863, Sept. 14	16 25	1 0 35	+9 56·7
15	10 2	1 0 14	+9 52·2

The Planet was of the 10th magnitude. Observations, Oct. 4, at Leipsic, and Oct. 6, at Vienna, are published, *Ast. Nach.*, Nos. 1440 and 1441; and the observations, from Sept. 14 to Sept. 23, by Mr. Watson, at Ann Arbor, *Ast. Nach.* No. 1442, where the following elements are given:—

Epoch 1863, Sept. 19 Greenwich M.T.

$$M = 333^\circ\ 50'\ 17''\cdot8$$
$$\pi = 45\ 35\ 56\cdot0 \quad \Big\} \text{ Mean Equinox } 1863\cdot0.$$
$$\Omega = 206\ 37\ 17\cdot4$$
$$i = 4\ 38\ 27\cdot1$$
$$\varphi = 11\ 19\ 25\cdot1$$
$$\log a = 0\cdot388320$$
$$\log \mu = 2\cdot967527$$
$$\mu = 927''\cdot955$$

A communication has been received from Prof. J. Zech, of Tübingen, containing the Statutes of the Astronomical Society founded at Heidelberg on the 28th of Aug. 1863. The Society is declared to be a union of astronomers and friends of astronomy, with a view to the advancement of that science, particularly in the carrying out of such works as require a systematic co-operation of many persons. Above all the Society aim at the drawing up, with united strength and according to uniform principles, of elements of reduction and auxiliary tables for the effective execution of the preliminary labours which many investigations require. The Society seeks also to promote, by seasonable support, in important

investigations, the co-operation of several labourers, or the means of enabling individuals to devote themselves to such for several years. The business language is German, but the membership is limited to no nationality. The seat of the Society, as the place of meeting, and of issue of its publications, is at first Leipsic. The Council is as follows:—

J. Zech, Professor at Tübingen, President.
F. Argelander, Professor at Bonn.
O. Struve, Director of the Observatory at Pulkova.
C. Bruhns, Professor at Leipsic, Vice-President.
E. Schönfeld, Professor at Mannheim, Secretary.
W. Förster, Dr., Observer at the Berlin Observatory, Secretary.
J. Zöllner, Dr., at Leipsic, Treasurer.

All the members of the Council are prepared to receive declarations from persons desirous of joining the Society.

Rectification.

Mr. Howlett writes, that by an error on his part, it is stated on page 267, fifth line from the top, in the supplementary *Monthly Notice*, vol. xxiii. No. 9, that the divided glass diaphragm employed in his record is ruled off into $\frac{1}{100}$ths of an inch. It is really ruled off into $\frac{1}{200}$ths.

RECENT PUBLICATIONS.

Positiones Mediæ Stellarum fixarum in Zonis Regiomontanis a Besselio inter + 15 *et* + 45 *Declinationes observatarum ad annum* 1825 *reductæ et in Catalogum ordinatæ, auctore Maximiliano Weisse, Directore quondam Speculæ Cracoviensis. Jussu Academiæ Imperialis Petropolitanæ edi curavit et præfatus est Otto Struve Speculæ Pulcovensis Director.* 4to. Petropoli, 1863.

The title is a sufficient explanation of the contents of this Catalogue, a continuation of the former Catalogue, extending from Decl. — 15° + 15°, edited by W. Struve in 1846.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIV. *Dec.* 11, 1863. No. 2.

G. B. AIRY, Esq., President, in the Chair.

Charles George Talmage, Esq.;
was balloted for and duly elected a Fellow of the Society.

Some Remarks on the Telescopic Appearance of the Exterior Envelope of the Sun, and of its Spots. By the Rev. W. R. Dawes.

The recent increase in the number of powerful telescopes armed with the means of observing the Sun without danger to the eye or to the dark glasses, has caused a corresponding increase in the number of observers of solar phenomena. These, when carefully scrutinised with large apertures and high powers under suitable atmospheric circumstances, are so wonderfully different in their appearance from those presented by the diminished apertures formerly and necessarily in use, that it would not be very surprising if some observers, unaware of what had previously been seen and described, should imagine that the phenomena revealed by their newly acquired and powerful telescopes were really *new discoveries.* And this is what, there is good reason to believe, has in some instances actually occurred. Such a mistake may also be more likely to be made when a *new name* has been applied by some recent

observer to an appearance long familiar to others, though previously unnamed. A name, no doubt, has the advantage of affording a convenient handle whereby to grasp the thing described; but, unless it is very correct and appropriate, it conveys an erroneous impression of the appearance to which it is applied, and may become far more injurious than advantageous. It seems to me, therefore, to be desirable to direct attention to what has been long ago observed and described; and also to put on record some results of a pretty constant scrutiny of solar phenomena with powerful and excellent telescopes during the last twelve or fifteen years.

The mottled appearance of the solar surface requires no very large amount of optical power to render it visible. I have often observed it with a good refractor of only 2½ inches aperture and a power of 60. Examined with a large aperture, such as 6 or 8 inches, it becomes evident that the surface is principally made up of luminous masses imperfectly separated from each other by rows of minute dark dots,—the intervals between these dots being extremely small, and occupied by a substance decidedly less luminous than the general surface. This gives the impression of a division between the luminous masses, especially with a comparatively low power, which, however, when best seen with high powers, is found to be never complete. The masses thus incompletely separated are of almost every variety of irregular form;—the rarest of all, perhaps, being that which is conveyed to my mind by Mr. Nasmyth's appellation of "*willow-leaves;*" viz. *long, narrow,* and *pointed.* Indeed, the only situation in which I have usually noticed them to assume anything like such a shape, is in the immediate vicinity of considerable spots, on their *penumbræ*, and frequently projecting beyond it irregularly for a small distance on to the *umbra;*—an appearance with respect to which, in my "*Description of a new Solar Eye-piece,*" which I read before the Society in April 1852 (see *Memoirs*, vol. xxi. p. 161), I employed the following expressions:—"The interior edge of the penumbra frequently appears extremely jagged; the bright ridges on its surface, which are directed nearly towards the centre of the spot, being seen projected to irregular distances on to the cloudy stratum [or *umbra*], and looking much like a piece of coarse thatching with straw, the edge of which has been left untrimmed." After nearly twelve years of careful observations of the same phenomena, I do not think that I could improve upon this description.

Of the general appearance of the Sun's surface, I have met with no more accurate description than is contained in Sir John Herschel's "*Outlines of Astronomy,*" Art. 387, where he says, "The part of the Sun's disk not occupied by spots is far from uniformly bright. Its ground is finely mottled with an appearance of minute dark dots or *pores*, which, when atten-

tively watched, are found to be in a constant state of change. There is nothing which represents so faithfully this appearance as the slow subsidence of some flocculent chemical precipitates in a transparent fluid, when viewed perpendicularly from above." The only part of this admirable description which I should be at all disposed to modify is that relating to the "constant state of change" in the *pores*. The plan I have uniformly adopted in the minute scrutiny of the solar surface has been, to employ very small perforations in the diaphragm-plate of my solar eye-piece (viz. from 20″ to 60″ in diameter), using the highest magnifiers which circumstances would permit. In this way I have frequently kept the same luminous masses and the intervening pores in view for an hour or two together; and have rarely observed any decided change in either, even when well seen with powers from 400 to above 600. Yet very slight atmospheric tremours suffice to give an appearance of almost perpetual variation; and unless the view is confined within the narrow limits of a very small field, the eye is apt to become confused in its judgment of the relative positions of objects so minute and delicate.

A striking exception, however, to this comparative quietude is found in the immediate vicinity of spots which are either rapidly enlarging or closing. It is under these circumstances especially that the luminous masses are found to become more elongated. This is also more remarkably the case when they are *preparing for a rush across a chasm*, and thus forming those luminous bridges which so often intersect considerable spots. The point from which such a rush is about to be made is often indicated by a greater crowding together towards that place, and a general inclination of the longer axis of each of the elongated masses in that direction; which might, I imagine, be well exemplified by such chemical precipitates as Sir John Herschel alludes to, if they were about to flow through a narrow spout or opening in the vessel containing them. I have had my attention thus specially directed to such a point at the edge of a spot; and, having placed it in a very small field of view, have watched with great interest the formation of the first portion of the bridge. In this process, the luminous masses have the appearance of bits of straw, all lying nearly in the same direction, though sometimes rather obliquely to the line of the bridge: the sides of the bridge having a jagged appearance arising from the unequal length of the pieces which compose it. It is a remarkable fact that these bridges are always formed by the bright streaks of the outer stratum, such as are seen projected on the penumbra, without any admixture of the less luminous second or penumbral stratum. Such, at least, is the conclusion I have arrived at after frequent and very careful scrutiny. And so intense is the light of such a line, however narrow, that, when projected on to the umbra of a large spot, its superior brightness usually prevents the

cloudy stratum from being discerned at all, even when the umbral portion only is included in the small field of view.

With respect to the highly interesting subject of the origin or cause of the solar spots, I wish to direct special attention to the *black nucleus* which in most of the large and symmetrical spots is found to occupy some part of the *umbra*. I cannot but regret that, though nearly twelve years have elapsed since I announced the frequent existence of such a black spot, and that it would be highly desirable to restrict to it the term *nucleus*, yet I believe the distinction has never been made by any other observer; the *umbra* having been universally, so far as I know, designated as the *nucleus*, and the existence of the true black nucleus within it either forgotten or ignored. My earnest desire that the distinction should be recognised and established does not arise merely from the conviction that accuracy is preferable to inaccuracy, and that where a decided difference really exists it is desirable to express it by a difference of terms; though I should imagine that such a ground would generally be deemed sufficient of itself. A still more cogent reason is found in the fact, that the whole series of my observations for twelve years past has led me to the conclusion that generally, if not universally, the existence or absence of the black nucleus is sufficient to determine, or at least to throw much light upon, the *origin* of the spot; and that the origin of those in which the nucleus exists is widely different from the origin of those from which it is absent. To do justice, however, to this view of the subject would unduly lengthen the present paper; and I, therefore, hope to be able, in a future communication, more fully to lay before the Society the conclusions at which I have arrived.

Hopefield Observatory, Haddenham, Bucks,
9th Dec. 1863.

On the Motion of the Solar System of Space.
By E. J. Stone, Esq.

On 1859, March 11, the Astronomer Royal read before the Society a paper "On the Movement of the Solar System Space;" the great novelty of his method was that no assumption was required of an approximate position of the apex of solar motion; the stars (in the second supposition upon which stress was chiefly laid) were supposed to have independent motions of their own, of any magnitude and in any direction, the only restriction introduced being that in our present ignorance the motion in one direction was as probable as that in any other

direction. Mr. Airy adopted M. W. Struve's scale of distance, estimated with respect to brightness. The method was first applied to 113 stars selected from Mr. Main's papers, on account of their having large proper motions; the result of the paper was to confirm previous investigations with respect to the direction of the supposed solar motion, but the velocity of the solar system obtained in this investigation was nearly six times as great as that obtained by M. Otto Struve, and the sums of the squares of the proper motion, corrected for the supposed solar motion, were not very greatly diminished.

The Astronomer Royal's method was next applied to all the stars contained in Mr. Main's papers on Proper Motion, amounting in all to 1167, the results were drawn up in a paper and read before this Society by Mr. Dunkin, 1863, March 13; the chief results obtained were to confirm previous investigations with respect to the direction of the supposed solar motion, and to obtain a satisfactory agreement, with respect to velocity, with M. Otto Struve's previous investigation, the sums of the squares of the corrected proper motions were however scarcely diminished at all.

The results of these papers of the Astronomer Royal must at least be allowed to have thrown some doubts upon our power of extracting numerical evidence of the existence of a motion of the solar system from our present determination of proper motions. In Mr. Main's papers on Proper Motion he has, for the stars with proper motions equal to or greater than $0^s{\cdot}02$ in R.A., and $0''{\cdot}3$ in N.P.D., computed the factors of solar motion on the supposition that the apex has for its R.A. 260°, and for its N.P.D. 55° 37′. Now this position agrees very closely with that assigned by the investigations of different astronomers, these stars are the same as those used by the Astronomer Royal in his first investigation, and the proper motions being considerable they can be roughly tested by modern observations *inter se*. I have first tested the proper motion, I found them on the whole so well borne out that, for my present purpose, I have used them intact.

The question I have proposed to myself is simply this:—Is it possible to determine the linear velocity of the solar system, so as to obtain any considerable diminution of the sums of the squares of the corrected proper motions?

The proper motions with which I am here dealing are all large; I shall take for my linear unit the mean distance of the group of stars under consideration, and suppose that on the whole the stars will be fairly distributed about this mean, or, in other words, I shall assume all the distances to be equal.

First including all the stars for which Mr. Main has calculated the solar factors—I find the sum of the squares of the proper motions in R.A. (expressed in seconds of time) to be equal to 0·525; the velocity of solar motion $=0''{\cdot}054$; the sum of the squares of the proper motions corrected for solar motion

=0·473; the greatest possible diminution, therefore, is to the whole only in the ratio of 52 : 525, or 1 : 11.

The velocity of the solar motion is here exceedingly small, and the reason is that the given factors of solar motion are not sufficiently accurate for stars near the pole; if we reject all stars within 10° of the pole, amounting in number to 13, we obtain the following results:—

Sum of squares of uncorrected proper motions in R.A.	= 0·464
Velocity of solar motion	= 0″·434
Sum of squares of corrected proper motions	= 0·381

and the diminution bears to the whole the ratio of 83 : 464, or 1 : 5½ nearly.

Next, for the proper motions in N.P.D.,

The sum of the squares of the proper motions uncorrected	= 48·539
Velocity of solar motion	= 0″·341
Sum of squares of proper motions corrected	= 44·862

and the diminution bears to the whole the ratio of 36/7 : 48539, or 1 : 13 nearly.

The close agreement of the values of the velocity of solar motion deduced independently from the consideration of the proper motion in R.A. and N.P.D. is satisfactory; if we used the mean value 0″·403 the diminution in R.A. and N.P.D. would be very nearly as great as those given above.

It appears, therefore, that by supposing the solar system to be moving with an angular velocity =0″·403 (the mean distance of the group of stars under consideration being taken as the unit) towards a point whose right ascension =260°, and N.P.D. =55° 37′, the sums of the squares of the corrected proper motions in R.A. can (for the group of stars under consideration) be diminished in the ratio of 1 : 5½, and the sum of the squares of the proper motions in N.P.D. in the ratio of 1 : 13.

This is omitting, for convenience of computation, stars near the pole.

In Mr. Dunkin's paper a very large number of small proper motions are included: an inspection of the books of computation shows that the curves of the squares of the small proper motions (on account of the correction applied being comparatively large) are in general increased by the application of the effect of the supposed solar motion. In that paper the distribution of distance is made solely according to brightness. It appears to me that, when small and large proper motions are used together, the sum of the squares of the corrected proper motion can be decreased but very little, unless some distribution of distance is made with respect to the magnitude of proper motion.

I think we may fairly conclude that so long as we confine

our attention to large proper motions, we have numerical evidence of a drift such as would be produced by the motion of the solar system in space, but that the numerical evidence is slight. Mr. Otto Struve and the Astronomer Royal both found the evidence stronger in the case of proper motion in R.A. than in N.P.D.; and this is very strongly marked in the present paper. It may, perhaps, not be unworthy of notice, that if Bradley's R.A. required a correction of the form x cos R.A. + y sin R.A., the apparent drift would be at once accounted for: the variation of the above formula from that required on the supposition of solar motion being exceedingly small for stars of considerable N.P.D.

Greenwich, 1863, *Dec.* 11.

On the Eclipses recorded in the Ancient Chinese Historical Work called Chun Tsew. By John Williams, Esq., Assistant Secretary.

In the account of the *Chun Tsew*, a Chinese historical work written by Confucius, given by Father Gaubil, one of the French missionaries to China, in his *Traité de la Chronologie Chinoise*, he mentions that it contains an account of several eclipses of the Sun. Ideler, also, in his paper, "Ueber die Zeitrechnung der Chinesen," printed in the *Transactions of the Berlin Academy*, speaking of the *Chun Tsew*, says, "What gives great interest to this work is the account of thirty-six solar eclipses observed in China, the first of which was on the 22d February, 720 B.C., and the last 22nd July, 495 B.C." As no further account of these eclipses is given in these works, and as there might be some difficulty in readily finding the particulars of them, should they be required, I have been induced to examine the copy of the *Chun Tsew* in my possession, and to extract the accounts of these eclipses as given in the original Chinese, and which I now beg permission to lay before the Society.

The *Chun Tsew* is said to be the only work really written by Kung Foo Tze, or, as we call him, Confucius; the other treatises attributed to him having been compiled by his disciples, either during his life, or, as in the last of them, some years after his death. It treats of the history of the Le Kwo, or Confederated Nations, into which China was divided during the Chow Dynasty, viz. between 1122 and 255 B.C.

The period of this history is from 722 to 479 B.C., being an interval of about 242 years, during the latter part of which Confucius flourished. It is more particularly devoted to the history of the small state of Loo, the birth-place of Confucius, where he passed a great portion of his life, and was advanced to the highest honours the monarch could bestow. It contains

the history of twelve princes of this state, with incidental notices of the other Confederated Nations. The number of the years of each reign is accurately determined, and the events are classed under the years in which they occurred. Each year is divided into four sections, under the four seasons—spring, summer, autumn, and winter—subdivided into months, and often with the days of the month distinguished by the characters of the cycle of sixty years, also used as ordinary numbers; to which, in my copy, are added copious commentaries by eminent writers.

The name *Chun Tsew* is said to have been given to this work from its having been commenced in spring and finished in autumn. I am, however, inclined to believe that it rather refers to the before-mentioned division of the years into seasons than to the time of the beginning and ending of the writing of the work. It is written in a very concise and severe style, being confined to the bare mention of facts, without comment or observation; and although to us, accustomed to the style of Gibbon or Macaulay, it would appear dry and uninteresting, it is much valued by the Chinese as a model of the ancient style of writing. It forms one of the *Woo King*, or Five Classical Books, without a thorough knowledge of which and of the *Sze Shoo*, or four books, no man can attain to any post of importance in the Chinese Empire.

The account of each eclipse is but little more than a brief mention of its occurrence at a certain time. As an example, I will give the particulars of the first, as recorded in the *Chun Tsew*. I must premise by observing that some of these particulars do not follow each other consecutively, as I quote them, but have to be gathered from parts of the preceding portion of the work, including the commentary. They read as follows:—"In the fifty-eighth year of the thirty-second cycle, in the fifty-first year of the Emperor King Wang, of the Chow Dynasty, the third year of Yin Kung, Prince of Loo, in the spring, the second moon, on the day called Kea Tsze, there was an eclipse of the Sun." This fifty-eighth year of the thirty-second cycle answers to 720 B.C.

Such is the general style of the announcement of these eclipses; but, as a constant repetition of precisely similar passages, differing only in the date and monarch, would perhaps be tedious, I have formed a table, in which I have confined the particulars to merely the cycle, the year of the cycle, the season, month, and day on which the eclipse occurred; to which I have added the years B.C., and also the months and days, according to our mode of expressing them. The years have been carefully computed, according to the method explained in my paper of June last, and verified by the Chinese Chronological Tables in the *She Ke*, or great body of historical annals in 300 vols., and also by similar tables recently published in Japan, about the year 1860. I may also state that the

present year, 1863, is the sixtieth or last year of the seventy-fifth cycle of sixty years, the first cycle commencing 2637 B.C. The days have been computed according to the method laid down by Ideler in the paper before referred to. They must, however, be considered merely as approximately correct.

No.	Cycle.	Year of Cycle.	Season.	Moon.	Denomination of Day.	Year B.C.	Month and Day.
1	XXXII.	58	Spring	2d	Kea Tsze	720	Feb. 22
2	XXXIII.	9	Autumn	7th	Jin Shin	709	July 17 (Total.)
3	,,	23	Winter	10th	1st Day of Moon.	695	Oct. 10
4	,,	42	Spring	3d	,,	676	April 15
5	,,	49	Summer	6th	Sin We	669	May 27
6	,,	50	Winter	12th	Kwei Hae	668	Nov. 10
7	,,	54	Autumn	9th	Kang Woo	664	Aug. 28
8	XXXIV.	3	Autumn	9th	Woo Shin	655	Aug. 19
9	,,	10	Spring	3d	Kang Woo	648	April 6
10	,,	13	Summer	5th	Kwei Hae	645	April 15
11	,,	32	Spring	2d	Kwei Hae	626	Feb. 3
12	,,	46	Summer	6th	Sin Chow	612	April 28
13	,,	57	Autumn	7th	Kea Tsze	601	Sept. 20 (Total.)
14	,,	59	Summer	4th	Ping Shin	599	Mar. 6
15	XXXV.	6	Summer	6th	Kwei Maou	592	June 16
16	,,	23	Summer	6th	Ping Yin	575	May 9
17	,,	24	Winter	12th	Ting Sze	574	Oct. 22
18	,,	39	Spring	2d	Yih We	559	Jan. 14
19	,,	40	Autumn	8th	Ting Sze	558	May 31
20	,,	45	Winter	10th	Ping Shin	553	Aug. 31
21	,,	46	Autumn	9th	Kang Seuh	552	Aug. 20
22	,,	46	Winter	10th	Kang Shin	552	Sept. 19
23	,,	48	Spring	2d	Kwei Yew	550	Jan. 5
24	,,	49	Autumn	7th	Kea Tsze	549	June 19
25	,,	49	Autumn	8th	Kwei Sze	549	July 18
26	,,	52	Winter	12th	Yih Hae	546	Oct. 13
27	XXXVI.	3	Summer	4th	Kea Shin	535	Mar. 18
28	,,	11	Summer	6th	Ting Sze	527	April 18
29	,,	13	Summer	6th	Kea Seuh	525	Aug. 21
30	,,	17	Autumn	7th	Jin Woo	521	June 10
31	,,	18	Winter	12th	Kwei Yew	520	Nov. 23
32	,,	20	Summer	5th	Yih We	518	April 9
33	,,	27	Winter	12th	Sin Hae	511	Nov. 14
34	,,	33	Spring	3d	Sin Hae	505	Feb. 16
35	,,	40	Winter	11th	Ping Yin	498	Sept. 22
36	,,	43	Autumn	8th	Kang Shin	495	July 22

The place where these eclipses were observed was possibly somewhere in the state of Loo. This state occupied a portion of the present province of Keang Nan, in the east of China, and was situated between the great rivers Yang Tze Keang and Hwang Ho. It extended along the banks of the latter river to its mouth, and was between the 32d and 34th degrees of north latitude, and from 115 to 120 east longitude.

I have but little to add to this account, save that two of these eclipses (viz. those of 709 and 601, Nos. 2 and 13) are recorded as having been total. The Eclipses of 542 and 549, Nos. 22 and 25 of the list, are stated to have occurred the month after the eclipse immediately preceding them.

In conclusion, I have to remark that I have merely given the data for these eclipses as they occur in the *Chun Tsew*, leaving it to those who are accustomed to this kind of work, and who may feel an interest in it, to ascertain the correctness of the recorded observations, and thus to test the general accuracy of early Chinese history.

Results of the Meridional Observations of Small Planets; Occultation of Stars by the Moon; and Phenomena of Jupiter's Satellites; made at the Royal Observatory, Greenwich, from June to October, 1863.

(*Communicated by the Astronomer Royal.*)

Angelina (64).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Sept. 22	11 55 22·1	0 0 43·86	88 13 14·87
29	11 21 59·8	23 54 51·96	88 49 32·32

Cybele (65).

Mean Solar Time of Observation.		R.A. from Observation.	N.P.D. from Observation.
	h m s	h m s	° ′ ″
1863, Sept. 11	11 51 47·9	23 13 47·02	95 46 34·72
16	11 28 49·8	23 10 27·96	96 11 49·81

All the observations of N.P.D. have been corrected for refraction and parallax.

Occultation of Stars by the Moon.

Day of Obs.	Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
1863, July 28	ξ^1 Sagittarii, disapp.	Dark	8 59 59·1	C.
28	ξ^1 Sagittarii, reapp. (*a*)	Bright	10 7 20·1	C.
Oct. 23	16 Piscium, disapp.	Dark	9 49 31·4	J. C.
30	χ^1 Orionis, disapp. (*b*)	Bright	9 43 36·0	D.
30	χ^1 Orionis, reapp.	Dark	10 47 14·4	D.

(*a*), The star faint; sensibly separated from Moon's limb when first seen. (*b*), The star very faint when near the Moon's limb.

The initials D., C., and J. C. are those of Mr. Dunkin, Mr. Criswick, and Mr. Carpenter.

Phenomena of Jupiter's Satellites.

Day of Observation. 1863.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
June 2	III.	Ingress, bisection	10 35 42·8	E.
4	II.	Occ. disapp. first cont.	9 33 28·1	C.
4	II.	,, last cont.	9 36 57·5	C.
29	II.	Occ. reapp. bisection	8 54 15·1	J. C.
29	I.	Ecl. reapp. (*a*)	9 46 46·5	J. C.

(*a*), The sky rather hazy; the image of the planet not good.

The initials E., C., and J. C., are those of Mr. Ellis, Mr. Criswick, and Mr. Carpenter.

New Elements of Leto (64). By Dr. Luther.

Epoch, 1863, Nov. $16^h·0$, Berlin M.T.

$$M = 86^\circ\ 41'\ 51''·6$$

$$\left.\begin{aligned} \varpi &= 345\ 10\ 5·0 \\ \Omega &= 44\ 53\ 14·3 \\ i &= 7\ 57\ 9·2 \end{aligned}\right\} \text{Equinox, 1863, Nov. 16.}$$

$$\varphi = 10\ 48\ 20·6$$

$$\mu = 764''·5970$$

$$\text{Log } a = 0·4443823$$

Elements of Comet V., 1863. By H. Romberg.

(*Communicated by J. G. Barclay, Esq.*)

Another comet was discovered by M. Tempel at Marseilles on Nov. 5th. It was visible to the naked eye, shining as bright as a star of the 5th mag. It appears as a condensated nebula, showing a tail about 2° long. The Comet has passed its perihelion, and is now removing from Sun and Earth, and will therefore become fainter. I calculated the following elements and Ephemeris, the latter of which will be useful for finding it.* It will probably be observed during a long time, but only in the morning hours.

T = 1863, Nov. 9·49923 Greenwich M.T.

π = 94° 46′ 10″·6 } Apparent Equinox, Nov. 13·5

☊ = 97 31 15·2 }

i = 78 6 46·5

log q = 9·849148

Motion direct.

The error of the mean place is

C — O.

$\Delta\lambda = +11''\cdot3$ $\Delta\beta = -1''\cdot6$

Mr. J. G. Barclay's Observatory, Leyton, N.E., Nov. 22, 1863.

Comets IV. and V., 1863. By H. Romberg.

(*Communicated by J. G. Barclay, Esq.*)

The Comet IV. was observed here as follows —

1863.	G.M.T. h m s	Comet's R.A. h m s	Log. $p\,\Delta''$.	Comet's Decl. ° ′ ″	Log. $p\,\Delta''$.	
Dec. 2	8 12 39	14 48 39·75	0·556	+46 11 22·2	0·904	*a*
6	6 50 45	15 20 54·21	0·781	+45 28 25·1	0·844	*b*
„	8 30 19	15 21 27·81	0·543	+45 27 6·9	0·905	*b*

The mean places of the stars of comparison are, according to Argelander,

		h m s	° ′ ″
1863·0	*a*	14 48 57·44	+46 13 49·1
	b	15 13 58·33	+45 30 59·2

This comet appears as an oblong nebula, strongly condensed in the middle. Its diameter is about 1′, and it shines now as

* The Ephemeris, extending from Nov. 26 to Dec. 26, 1863, is not printed.— Ed.

a star of the 8th mag. The Ephemeris I gave for this comet is now out very much, but I have not yet been able to get here three observations for a basis of a new calculation.

Of the Comet V. I caught only a hurried place on Dec. 3,

		Comet's R.A.	Comet's Decl.
	h m s	h m s	° ′ ″
1863, Dec. 3	6 16 17	15 46 35·8	+30 16 17

which agrees well with my Ephemeris. The comet appeared round, about 2′ in diameter. Its well-defined nucleus was as bright as a star of mag. 7. The tail appeared very faint, through clouds. This comet sets now about $7^h\ 51^m$ G.M.T., and may be well observed on evenings. Both comets will probably be observed during a long time. They seem not to have been observed before.

Mr. J. G. Barclay's Observatory, Leyton, N.E.
Dec. 9, 1863.

RECENT PUBLICATIONS.

Untersuchungen über die in der Nähe von Moskau stattfindende Local-Attraction, von G. Schweizer (Moskau, 1863, pp. 1–164), contains in detail the observations, and theoretical investigations grounded on them for determining the seat of the deficiency of mass in relation to the remarkable disturbance of the direction of gravity in the neighbourhood of Moscow; an account of which, as given verbally by M. Otto Struve at the meeting of the Society in April 1863, has already appeared in the *Monthly Notices*, No. 6, p. 185.

Jahresbericht am 14 *Juni*, 1863, *dem Comité der Nicolai-Hauptsternwarte abgestattet vom Director der Sternwarte* (*aus dem Russischen übersetzt*). *Anhang I. Statuten der Nicolai-Hauptsternwarte. Abhang II. Verordnungen des Ministers.* St. Petersburg, 1863. 8vo, pp. 1–48.

A more full account might properly be given of this Report by M. Otto Struve, presented in pursuance of the new statutes of the "Nicholas Head-Observatory" at Pulkova, and relating to the fifteen months which have elapsed since his definitive appointment as Director; but M. Struve, at the commencement of it, holds out the hope of giving, on the approaching solemnization of the twenty-fifth anniversary of the foundation,

a survey of the work of the Observatory during the quarter century of its existence. The Report is, as the Director remarks, drawn up on the model of those of the Greenwich Observatory. In the course of it the following list is given of Astronomical Memoirs recently published in the Memoirs of the Academy of St. Petersburg :—

1. O. Struve, Observations de la Nébuleuse d'Orion.
2. O. Struve, Observations faites à l'époque où le plan des anneaux de Saturne passait par le soleil.
3. Winnecke, Considérations concernant les observations méridiennes à faire pendant l'opposition prochaine de Mars, dans le but de déterminer sa parallaxe.
4. Oom, Vergleichung des Armagh-Catalogs von Robinson mit dem Aboer von Argelander.
5. Weisse, Positiones mediæ stellarum fixarum in zonis Regiomontanis observatarum. Pars secunda. Edi curavit et præfatus est O. Struve.
6. Winnecke, Beobachtungen des Mars zur Zeit der Opposition 1862 ;

besides a Memoir in Russ by the adjunct-astronomer Smysloff, intended to appear as a separate work, and two Memoirs in the same language in the Proceedings of the Topographic Department ; and also a Memoir by Herr Gylden "Berechnung der Bahn des Cometen 1860. II.," ordered to be printed.

Ueber die Parallaxe des Sterns LL. 21258. Von A. Krüger. *Ueber die Parallaxe des Sterns Oeltzen N*: 0, 17415,6. *von demselben. Aus den Abhandlungen der Finnischen Societät der Wissenschaften.* Helsingfors, 1863.

The results deduced from the observations recorded in these two memoirs by M. Krüger are as follows :—

Annual Parallax of LL. 21258 in relation to the two comparison stars is $= 0''\cdot260 \pm 0''\cdot020$.

Annual Parallax of Oeltzen, N : 0, 17415,6 in relation to the two comparison stars is $= 0''\cdot247 \pm 0''\cdot0211$.

William Herschel's Verzeichnisse von Nebelflecken und Sternhaufen, bearbeitet von Arthur Auwers ; Königsberg, 1862, fo. pp. 1–77, (forms part xxxiv. of the *Königsberg Observations.*)

The work is a reproduction of the three Catalogues of Nebulæ and Star-clusters given by Sir W. Herschel in the

Philosophical Transactions for the years 1786, 1789, and 1802, and which contain the descriptions of about 2500 of these objects, together with their distances from neighbouring stars, arranged in the order of discovery or determination of the positions. The absolute positions have been previously calculated by Bode and Caroline Herschel, and more recently as to two of the eight classes by D'Arrest, but a new and complete reduction of the observations appeared very desirable, and M. Auwers has now availed himself of the opportunity given to him by Prof. Luther for the publication in the *Königsberg Memoirs* of the results calculated by him originally for his own use, about seven years ago.

The objects are referred to by their class and number, as in the original Catalogues, but they are now arranged in the order of their Right Ascensions, Sir W. Herschel's descriptions being given in English in the words and with the symbols originally made use of. The calculated positions are compared with those obtained by Sir J. Herschel in his Slough observations, and there are also references to these observations, and to the Catalogues of Lacaille, Messier, D'Arrest, and Laugier. There is also a reduction of the Catalogues of Messier and Lacaille, and of the positions of some subsequently discovered Nebulæ.

Instrument for Sale.

A first-rate Equatoreal Refractor, by Alvan Clark. The object-glass has a clear aperture of 8¼ inches, and a focal length of 109¾ inches. The driving clock can be regulated to the greatest nicety by its pendulum vibrating half seconds; and has often kept a wire on a star for an hour together. The telescope can be kept upon any object whose declination does not exceed 45° North, throughout its whole course, without turning over the telescope to the other side of the polar axis. For further particulars, address Rev. W. R. Dawes, Hopefield Observatory, Haddenham, Thame.

Instrument for Sale.

A Transit by Troughton and Simms, on the same construction as that of Greenwich, 8-feet focal length and 5·3-inches aperture. It has all the usual appendages, and is in excellent order. For further information, reference may be made to A. Graham, Esq., Markree Observatory, Colooney.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Warren De La Rue, Esq., Vice-President, in the Chair.

E. J. Kennelly, Esq., H.M. Dockyard, Bombay;
Rev. W. A. Darby, Manchester;
Alexander Freeman, Esq., St. John's College, Cambridge;
E. J. Henessey, Esq., Head Master, Navigation School, Aberdeen;
G. T. Sadler, Esq., 51 Oakley Road, Islington;
Frederick Smith, Esq., 18 Mornington Road, Bow;
Lieut. A. W. Stiffe, late of H. M. Indian Navy;
W. H. Dobie, Esq., M.D., Chester;
A. Brothers, Esq., 110 Upper Brook Street, Manchester; and
J. W. Mulcaster, Esq., Woolwich Common;

were balloted for and duly elected Fellows of the Society.

On the Calculation of an Optical Effect of Atmospheric Refraction. By Professor Challis.

In the *Monthly Notices* for Nov. 13, 1863, the Astronomer Royal has called in question an optical effect which I have attributed to a supposed lunar atmosphere, in my paper "On the Indications of Atmospheres by Phenomena," contained in the *Monthly Notices* for June 12, 1863. The argument of the Astronomer Royal, and the inference he has drawn from it, demand from me a reply; which I beg to be allowed to publish through the same medium.

It is assumed at the commencement of the argument that if a part of the Sun's disk be viewed through a refracting medium, such as a lunar or any other atmosphere may be supposed to

be, the effect produced would be "analogous to that of a convex lens;" and accordingly Mr. Airy proceeds to investigate the effect mathematically on that assumption. The curved figure by which the outline of the refracting medium is represented, is not, however, a section of a lens, but the transverse section, coincident with the plane of refraction, of two intersecting cylindrical surfaces, and the different inclinations of the rays are wholly in that plane. On these suppositions, which are legitimate, the reasoning is conducted without introducing any approximate process; and as nothing depends on the constancy of the values of the radii of curvature r and ρ, these may be supposed to vary from one pencil to another in any arbitrary manner. Thus there is no limitation as to the form of the outline of the refracting medium (except that it be convex outwards), and so far the demonstration is sufficiently comprehensive. It may even apply to the case of vision through a mean atmosphere of uniform density and given altitude. The result arrived at is, that the brightness of an object seen through the refracting medium is the same as if no medium were interposed, loss of light in the transmission being left out of account.

But there remains a fatal objection to the application of this result to the question under consideration. No account has been taken of the variation of the refractive index (n) in passing from one point to another of the medium. In every investigation respecting the courses of rays in atmospheres, such variation of the refractive index is an indispensable consideration. If the atmospheric refraction were analogous to that through a lens, or if it were correctly represented by the refraction through a mean atmosphere, the deviations of the rays would be greater the further their courses are from the surface of the globe on which the atmosphere rests. Whereas in the actual case of an atmosphere diminishing in density with the height, the higher the courses the less the deviations. For these reasons I am unable to accept the demonstration of the Astronomer Royal as applicable to the case of viewing the Sun's disk through an atmosphere.

The assertion which has been thus controverted may, I think, be maintained by the following considerations. The disk of the Sun, as seen by a spectator on the Earth's surface, is diminished by the effect of atmospheric refraction, the lower limb being more raised than the upper, and the vertical diameter and parallel ordinates being consequently diminished, while the horizontal dimensions remain unchanged. Hence if no light were lost by passage through the atmosphere, the brightness of the Sun, so far as it depends on the number of pencils in a given space, would be increased by the refraction. But to calculate the actual brightness, we must also take account of the effect of the refraction relative to the number of rays of a given pencil which enter the pupil of the eye. Con-

ceive the atmosphere to be divided into small parallel strata of uniform thickness and density, the density varying by small gradations from one stratum to the next. Then if μ be the refractive index for rays passing from any stratum to the contiguous denser stratum, and θ be the angle of incidence, and θ' that of refraction, of the axis of a pencil originally proceeding from a point of the Sun's disk, we shall have $\sin\theta = \mu \sin\theta'$, the index μ applying only to refraction between these two strata. Let another pencil proceed from a point of the Sun's disk distant by a small interval from the first point, and let the two pencils be subject to the condition of intersecting at the optical centre of the spectator's eye. To pass from one pencil to the other, so far as regards the two strata under consideration, we have only to vary θ and θ' in the above equation, and we shall thus find that

$$\frac{\delta\theta'}{\delta\theta} = \frac{\cos\theta \sin\theta'}{\cos\theta' \sin\theta}.$$

If now $\delta\theta$ represents the convergency of the two pencils in the rarer stratum, $\delta\theta'$ will represent their convergency after entering the denser; and it is evident, since θ is greater than θ', that $\delta\theta'$ is less than $\delta\theta$. The effect of this refraction is, therefore, to diminish convergency in the ratio of $\cos\theta \sin\theta'$ to $\cos\theta' \sin\theta$, and thus to increase the brightness of the object in the inverse proportion. But, as is evident from simple geometrical considerations, the refraction at the same time increases, in the ratio of $\cos\theta'$ to $\cos\theta$, the breadth of a pencil of rays proceeding originally from a single point of the Sun's disk, and consequently in that ratio diminishes the number of rays of the pencil which pass through a given aperture, as the pupil of the spectator's eye. Thus there will remain an increment of brightness in the ratio of $\sin\theta$ to $\sin\theta'$. The same effect will be produced at the common boundary of each two contiguous strata, so that the effect of the total refraction is to diminish the angle which a given portion of the Sun's disk subtends, and to increase the brightness, but in a ratio less than that of the diminution of apparent magnitude. These effects will clearly be greater the less the altitude of the Sun. It may be also remarked that the above reasoning respecting the refraction at the common boundary of a rarer and a denser medium, is strictly applicable to the case of vision under water, and shows that while the refraction in this instance causes external objects to be crowded together, it increases their brightness. The case of a spectator on the earth's surface viewing a heavenly body is certainly analogous to this.

For the sake of simplicity I have hitherto omitted the consideration of the curvature of the atmospheric strata. By taking it into account the above effects will be in some degree modified, but without alteration of the result. In fact,

I have found that when the curvature is included in the investigation, the expression for the ratio of the convergencies is the same as that above with the factor

$$1 - \frac{r \sin(\theta - \theta')}{R \cos^2 \theta' \sin \theta}$$

in the denominator, R being the radius of curvature of the stratum, and r the distance of the point of convergence of the refracted pencils from the point of refraction. This factor, so far as it is significant, increases the ratio of the convergencies, and, consequently, diminishes the brightness. But, at the same time, the number of rays which pass through a given aperture, at the distance r from the point of refraction, is increased in the very same proportion, so that the increase of brightness in the ratio of sin θ to sin θ' still remains.

Analogous reasoning applies to the case of the two pencils, when subject to the condition of converging to the eye of a spectator situated out of the atmosphere at a great distance; which is that of viewing the Sun through a supposed lunar atmosphere. The course through the atmosphere of the pencil from the lower of two points on the Sun's disk is *throughout* more curved than that from the upper, and their convergency is, therefore, continuously diminished by the refraction. In the first half of the course the bending is produced by passing from rarer to denser strata, and in the other half it is produced in the same direction by passing from denser to rarer. But while in the first half the breadth of a given pencil is continually increased by the refraction, in the second half it is continually diminished by the same gradations. Consequently the diminution of apparent magnitude of a given portion of the disk is wholly effective in producing an increase of brightness. The foregoing reasoning, which fully takes account of the variation of the density of the atmosphere, appears to establish sufficiently the correctness of the assertion which has given rise to this discussion.

I take the opportunity for adverting to an experiment bearing on the present subject, which was submitted to me by Mr. De La Rue through Professor Stokes, and is probably alluded to by Mr. Airy at the end of his communication. A photograph of one of the phases of the Solar Eclipse of 1860, obtained from the original negative taken by Mr. De La Rue in Spain, exhibited a very narrow band contiguous to the Moon's limb, very slightly more luminous than the adjacent parts of the Sun's disk. In order to test whether the band was due to contrast, Mr. De La Rue had divided a portion of the photograph from the conterminous black ground, that by lifting it a piece of white paper might be passed underneath it to cover the blackness. When this was done I certainly could not see the band along the edge of the separated portion, although I was able to see it along the edge of the remainder.

I am disposed to attribute the disappearance to the circumstance that the contiguous whiteness of the paper disqualified the eye for appreciating the slight excess of light of the band, which it was difficult under any circumstances to distinguish. To test this explanation, I applied a straight edge of the paper tangentially to the convex side of the band, leaving the black ground uncovered, and thought I perceived the band to be less conspicuous the more it approached the paper. Further, I remarked that, contrary to what might be expected if the phenomenon were due to contrast, no band was visible along the Sun's limb. The absence is hardly to be accounted for by the greater dullness of the parts of the Sun's disk contiguous to the limb, because the same circumstance did not appear to obliterate the band contiguous to the Moon's limb. Professor Stokes, however, informed me that he saw a band along the Sun's limb as well as one along the Moon's, stating at the same time that he looked at the photograph at the distance of several feet. On doing the same thing I also saw both bands, and found, besides, on increasing the distance, that they increased in magnitude, and even exhibited diffraction colours. But on looking at the photograph with a small telescope the bands entirely disappeared; which proves that this phenomenon has its origin in the eye of the observer, being due to an imperfect adaptation of the sight to the distance of the object looked at. Moreover these bands, after continually diminishing as I approached the photograph, wholly disappeared at the distance of most distinct vision, at which alone the band which is the subject of this inquiry was at all discernible. On making further experiments on this species of eye-diffraction I ascertained that the circumstance most favourable for perceiving it, was looking at the common boundary of two adjacent spaces, one black and the other white. I have mentioned these facts because they appear to me to account sufficiently for the band which, as Mr. Airy states, may be seen to precede the edge of a piece of black cloth swung upwards between the eye and a moderately illumined whitish wall. The band of the solar photograph is distinguished from this by not being discernible in any *casual* manner, requiring, on the contrary, the most careful attention to the circumstances most favourable to distinct vision.

From the foregoing considerations, and from the fact that the phenomenon in question independently attracted the attention of Professor Stephen Alexander and Mr. De La Rue, I seem justified in saying that there is *primâ facie* evidence of its being something extraneous to the observer, and that observations of it in future solar eclipses will be highly interesting, more especially if it is ascertained to be exclusively visible along the Moon's limb.

Cambridge, January 6, 1864.

Further Remarks on the Telescopic Appearance of the Exterior Envelope of the Sun, and of its Spots. By the Rev. W. R. Dawes.

Before proceeding to consider the character and cause of the Solar Spots, it may be desirable to recur to some points connected with the telescopic appearance of the general surface, in addition to the remarks contained in my former paper on the subject.

In speaking of the less luminous interstices between the intensely luminous masses, forming together the general surface of the Sun, I have used the term *pores* for the extremely minute openings found in them. This I adopted from Sir John Herschel's description of the phenomena. It may, however, be necessary to guard against the supposition that these pores are always, or even usually, quite *round*. When best seen under circumstances which are unfortunately of very rare occurrence, they sometimes have rather the appearance of very small *fissures*,—such as might be caused by several exceedingly small dots running together into one short dark line, which is never *black*, but at most of a *dark gray*. I have been strongly impressed with the conviction that their *visibility* varies greatly at different times, even under circumstances too similar to account satisfactorily for the difference. Indeed, I have on some occasions been able to find none, or scarcely any; while, on others, not remarkably more favourable, they have come out by pretty steady views in a sufficiently certain and positive way.

Perhaps this variation in their visibility may be connected with another fact to which I should wish to draw special attention; namely, that *the proportion of the area of the less luminous spaces* (in which the pores are sometimes found) *to that of the most luminous masses, is subject to very considerable change.* Though they are at no time very small or narrow, yet of late they have struck me as occupying a decidedly greater space than I used to assign to them. If such changes really exist, and are also great, they would seem necessarily to produce differences in the quantity of solar light, and might thus place our Sun among the slightly variable stars, and also afford a plausible way of accounting for the variation in others. I have indulged the hope of being able to give a more decided opinion as to the actual proportion between the masses of different luminosity; but I can only say that at the present time my impression is, that the area occupied by the less luminous masses is even *greater* than that of the brighter ones; but that the proportion in different parts of the Sun is by no means always the same at the same time. These conclusions may, however, be considerably modified by better and more continuous observations (with this special object in view) than I

have been able to procure, especially during the last eight months. Might it not be reasonably expected that *photography* would here step in most efficiently to our assistance? As an approximation to what is wanting, I may refer to the very remarkable print of the Sun's surface from an electro-copper block obtained from the original negative taken by Mr. Warren De La Rue. The dark interstices in that plate seem to represent the less luminous portions of the surface, which may possess far less photographic power. How such a picture can be reconciled with the idea of an interlacing of an infinite number of willow-leaf-shaped masses of equal brightness, I am at a loss to conceive. The brighter parts are in that picture, as I have always seen them, of all imaginable forms, and could not possibly be produced by any such interlacing.

Another feature of these masses of different brightness may here be adverted to. They certainly give the impression of an undulating surface, much like that of an orange, or of a fine head of cauliflower when becoming a trifle loose. The brightest masses seem to be higher and therefore thicker than the others; the brightness of the lower and thinner parts being modified by the decidedly inferior luminosity of the second or penumbral stratum, which is, as it were, partially seen through it. This idea would seem to be greatly strengthened by the fact, that the remarkably bright streaks called *faculæ* are undoubtedly *ridges*, being elevated to a considerable extent above the general surface: of which I have had ocular demonstration on more than one occasion by their actual projection at the limb of the Sun, like an irregular range of hills at the edge of the Moon; as well as by their frequently, either partially or entirely, concealing openings which have soon been brought into view by the Sun's rotation.

However plausible this conclusion may appear, there are two facts which strongly militate against it. One is, that the less luminous masses are seen *to the very edge of the Sun*, both near the equator and at the poles, except when near the equatoreal edge they are concealed by obvious faculæ. This seems to be quite inconsistent with their being to any considerable extent depressed below the brighter masses; sufficiently so, at least, to produce so great a difference in their brightness. The second fact is, that the boundaries between the two kinds of masses referred to are *too abrupt*, and the change of brightness too sudden, to allow of anything like gentle undulations. The sides of the brighter portions must be almost precipitous to produce the views we obtain of them under the finest circumstances.

From these facts I have been led to the conclusion, that the masses of inferior brightness, though self-luminous like the brighter, are *so far different in their nature that the two do not readily mingle together*, and that it is only at their confines

that they do so at all; and also that their surfaces are on the whole at the *same level*,—the principal exceptions being found in the *faculæ*, which are formed entirely of the brighter portions. Those which are less luminous may on that account be more transparent, and perhaps also less dense, and therefore more readily permitting the escape of some non-inflammable gas producing the pores or fissures occasionally found in them.

The apparent *mottling* on the Sun's surface is obviously produced by the juxtaposition of the more and less luminous masses or clouds; and I must confess myself quite unable to perceive how such a condition can be compatible with a pretty uniform distribution of luminous masses of any one shape or degree of brightness. The fine mottling is a certain fact, easily verified with a very moderate amount of instrumental aid, and subject to no suspicion of being an optical deception. It certainly seems to me therefore, that a proper degree of caution may well make us hesitate to admit as a fact any appearance obviously incompatible with it,—only rarely seen by very few, —and never once perceived by others during years of the most careful scrutiny, under the most favourable circumstances, and with optical means perfectly adapted to the purpose.

We may now proceed to consider the *cause and mode of formation of the different kinds of solar spots.*—Any one accustomed to observe them, even in the most cursory way, must have been struck, not only with the endless variety of their forms, but also with an essential difference in their character. For our present purpose they may perhaps be most suitably divided into two classes, the *profound* and the *superficial.*

1. The *profound.*—In this class I should include those which give evidence of involving all the visible envelopes, the disturbance being observable through them all, and down to what appears to be the body of the Sun itself. Among the *envelopes* I do not include any exterior to the photosphere, but restrict the term to the *three* which I have already referred to, but will here more particularly specify.

The *first* in order from the body of the Sun outwards is what, in my "*Description of a New Solar Eye-piece,*" I pointed out as being really an envelope, and not the body of the Sun, as it had been previously accounted, and as such had been generally called the *nucleus*, when it occupied the middle of a spot. By using a field of view sufficiently small to exclude even the *penumbræ* of pretty large spots, and using as light a glass as the eye can comfortably bear, a mottled appearance is perceived in this stratum,—the degree of darkness being by no means uniform, and suggesting the idea that the surface we see is far from level; the lighter parts being probably the most elevated, and feebly reflecting the light received from the self-luminous strata above it. To this lowest envelope I applied the term, *cloudy stratum*, forming the *umbra* of a spot. In all

spots which are tolerably symmetrical, and large enough to admit of accurate scrutiny, the *umbra* will be found to be perforated near its centre by a *perfectly black hole*, which I regard as the true *nucleus*. The form of the umbra usually approximates to that of the nucleus, being nearly round when the nucleus is so; and when the nucleus is much elongated, or when some part of it greatly varies in shape from the rest, the general form of the umbra is almost constantly found to partake of a similar irregularity. A very remarkable feature of this cloudy umbra is, that at the edge next to the nucleus is it usually decidedly less dark than elsewhere, giving the impression of being there more elevated, or heaped up. This is especially the case with spots which have only recently broken out, or are in the process of enlargement. On the contrary, when a spot is closing up, this feature disappears. Occasionally some very small bright specks are visible on this stratum; but I believe they do not belong to it at all, but are minute portions of one of the self-luminous strata above it.

The *second* or *middle* envelope constitutes the *penumbra* which usually attends all spots of considerable size and symmetrical forms. It possesses, I think, all the characters of self-luminosity, though its brightness is far inferior to that of the outer envelope. In most cases, lines of much greater brightness are projected upon it, which obviously belong to the outer one, or photosphere. But occasionally some parts of the penumbra, especially where it is very broad, are decidedly brighter than the rest, and these are rarely, if ever, overlaid with the bright lines from the photosphere. They give me the impression of belonging, not to the penumbral stratum at all, but to the *less* luminous portions of the photosphere projected on to the penumbral stratum. At the interior edge of the penumbra we generally find an appearance (especially when spots are enlarging) very similar to what I have noticed above as being found at the interior edge of the umbra; namely, an increase of brightness as compared with the rest of the penumbra; the light decreasing from its inner to its outer edge, where it is almost always perceptibly less than in any other part. That this is not the effect of contrast with the adjacent far brighter photosphere is proved by excluding that stratum from a small field of view. The impression is the same as in the case of the umbra,—that the inner edge of the penumbra is heaped up, and thicker than any other part of it.

The *third* envelope, or *photosphere*, affords the same kind of evidence of being heaped up, or rolled back on itself at its interior edge adjoining the outer edge of the penumbra. As I have stated above, it is not unfrequently *seen* to be so when spots of considerable size are near the Sun's limb. The elongation of the brighter self-luminous masses, as seen projected on the penumbra, and on some special occasions at its

outer edge, in the formation of luminous bridges, has been adverted to in my paper recently presented to the Society.

These remarks on the peculiar characteristics of the three envelopes will suffice to show on what evidence my conclusions rest respecting the manner in which these *profound* spots are formed, and which may be thus described.

An immense volume of some non-inflammable gas, discharged with prodigious force from the body of the Sun by volcanic or some similar agency, bursts through the *cloudy stratum*, rolling back on all sides the displaced portion of that stratum, and producing the heaped-up appearance at its inner and lighter edge. The *black hole* produced in the stratum by this volcanic eruption forms the *nucleus* of the spot.

Having passed through the cloudy stratum, the evolved gas comes within the influence of the heating power of the self-luminous penumbral stratum; and being greatly expanded thereby, its increased volume removes a far larger area of this second stratum than of the first; thus laying bare a considerable portion of the upper surface of the cloudy stratum, and producing the *umbra* of the spot. Here, too, the rolling back of the removed portion causes a heaped-up and brighter appearance at the inner edge of the penumbra. Being still further heated and expanded by approaching the photosphere, a similar effect is produced upon this upper stratum, but to a far greater extent; and a much larger portion of the photosphere is thrown off on all sides, which, being, as before, rolled back upon the rest, gives the appearance of a heaping up of the luminous masses at the exterior edge of the spot.

Such is the process, or something like it, by which, as I have been led to conclude, these "profound" spots are formed. And it may here be remarked, that an additional argument for the *smaller density* of the *less* luminous masses seen in the photosphere (and before referred to) is found in the fact, that these seem to be almost entirely removed from above the penumbra; leaving in general only the elongated forms of the *brightest* self-luminous clouds of the photosphere, which, converging towards the nucleus, are seen projected on the penumbra, and extending often beyond its interior edge on to the umbra, like bright straws, as I have previously described them.

The *rotary* motion of a *profound* spot may be produced by the exploded gas having acquired a whirlwind sort of action, and thus carrying round the parts of the different strata affected by it in the same direction. But though I have observed this motion with certainty on two or three occasions, I believe it to be exceedingly rare,—at least in this class of spots.

2. The *superficial* spots.—These appear from the general tenour of my observations to be almost always produced by convulsions of some kind in the photosphere itself, or at a

small depth below it. But, from the extraordinary variety of the effects, I confess that I am not prepared to add anything to the suggestions already advanced as to the character of those convulsions, or the means by which they may be produced. Many of the spots which seem to be thus formed are remarkable for the immense extent of the *abrasions*, so to speak, of the photosphere, leaving large areas of the penumbral stratum exposed; but with none, or scarcely any, of it removed, so as to show the cloudy stratum, and thus to produce any *umbra* at all. In these cases, the exposed penumbral stratum has rarely any bright lines of the photosphere projected upon it. Another remarkable fact is, that at some times, for months together, there is a tendency to the formation of small superficial spots, in considerable number (as from 8 to 12) arranged nearly in an *elliptic* form, the longer axis being nearly parallel to the Sun's equator. I have occasionally observed more than one of these curious elliptic outbreaks in different parts of the Sun at the same time. And there is almost universally this singular feature attending the spots thus arranged; *the penumbra of each is expanded outwards from the centre of the ellipse.* Not unfrequently there is in such cases nothing but the penumbra exposed, no umbra being visible: and even when a small umbra is exposed, the interior edge of each spot is formed by the photosphere,—no penumbra appearing on that side. The whole appearance gives the idea of a prodigious and widely extended force emanating from the centre of the ellipse *at a level but little below the photosphere*, and acting so obliquely as to disturb that stratum only at great distances from the centre.

1863, *December* 25.

P.S.—I am indebted to the kindness of Mr. Birt for some extracts from an elaborate paper by Sir William Herschel, with which I was previously unacquainted. It forms a part of vol. xci. of the *Philosophical Transactions*, and was read on the 16th April, 1801. The observations of several years are referred to in it; and some of them are so remarkable, and so much to the point on the subject of the telescopic appearance of the general surface of the sun, that I think it will be highly desirable to quote a few passages most clearly expressing the conclusions to which that admirable observer had arrived.

On page 304 of vol. xci. he states that he uses the term *self-luminous clouds* "as a general expression for all phenomena of the sun, in what shape soever they may appear, that shine by their own light."

On page 287, referring to the "corrugations" all over the surface, which he considered to consist of elevations and depressions, he quotes some of his observations as follows:—

"Dec. 22, 1799. The whole disk of the sun is strongly indented."

"Dec. 23, 1799. The corrugations have a mottled appearance. I see the figure of the dark and bright places. Many of the dark places are not round, but a little extended in different directions, and appear to be lower than the bright places."

"Dec. 27, 1799. On examining some of the largest corrugations with a high magnifying power, I see plainly that the less bright parts or indentations are small openings; and that those dark places which are the coarsest show the opaque surface of the sun the best."

In another place he remarks, "The whole disk is very much marked with roughness like an orange. Some of the lowest parts of the inequalities are blackish."

"Jan. 31, 1800. The indentations are very uniform, but not round. It seems they admit of every possible shape."

"Feb. 4, 1800. The points, or rather darker coloured places in the punctulations, were of different figures, few of them being round.

"Feb. 18, 1800. Among all the corrugations I could hardly perceive any that were round. They were of all shapes, chiefly lengthened."

"Jan. 15, 1801. The low places of corrugations do not contain punctures, but seem to be irregularly shaped places of less luminous matter than the borders which inclose them."

On these passages it may be remarked, that the decided mention of pores or punctures on some occasions, and the statement on others that there were none to be seen, strongly supports the conclusion I have arrived at, that the visibility of these minute dark points is subject to great variation.

Though Sir William seems to have entertained no doubt that the less luminous portions of the photosphere were considerably depressed below the brighter parts, yet he states that they were visible in every part of the disk; and in one place observes, under date Dec. 20, 1794, "I can follow the indentations from the centre up to the margin of the sun; but it requires great attention, as on account of the sphericity of the disk they become gradually less conspicuous the nearer we go to the circumference. I saw them equally well at the north pole of the sun."

As I have before remarked, the depression must be extremely small for this to be the case; and I should be far more inclined to attribute the increased difficulty in making out the spaces of different luminosity to the great fore-shortening of both near the sun's edge, though they might be on the same level.

Hopefield Observatory, Haddenham, Thame,
Jan. 6, 1864.

Sur le Coefficient de l'Equation Parallactique déduit de la Théorie. Par M. G. de Pontécoulant.

L'action de la Lune trouble le mouvement de la Terre autour du Soleil, et il en résulte par la réaction de la Terre sur la Lune des altérations dans quelques unes des inégalités dont le mouvement de la Lune autour de la Terre est affecté, altérations dont il faut tenir compte lorsqu'on veut atteindre à une grande précision. Ces altérations sont surtout sensibles sur les inégalités dépendantes de la parallaxe du Soleil, et en particulier sur le coefficient de l'inégalité qu'on a nommée *inégalité parallactique.* M. Plana, qui parait s'en être le premier préoccupé, a donné dans son grande ouvrage le moyen d'y avoir égard par une simple correction, introduite dans les formules ordinaires; et quoique l'exactitude de cette correction ait été révoquée en doute par Poisson, on est obligé de reconnaître qu'elle parait conforme aux principes d'une saine théorie. Cependant, depuis longtemps, je m'étais aperçu qu'au lieu de rapprocher les résultats de la théorie et de l'observation, la correction dont il s'agit semblait augmenter au contraire les différences qui pouvaient exister entre eux, et j'avais pensé que dans l'état actuel de la science, où le premier but qu'on doit se proposer est de faire disparaître entièrement ces légers écarts, il serait bon, peut-être, d'y renoncer, jusqu'à ce qu'une plus longue expérience ou des observations nouvelles aient fait reconnaître la nécessité d'y avoir égard. Les notes de M. Stone et de M. Hansen contenues dernièrement dans les *Monthly Notices** ayant rappelé mon attention sur cet objet, je saisirai cette occasion pour présenter sur ce point délicat de la théorie de la Lune quelques courtes observations.

Si dans l'expression de la longitude vraie de la Lune en fonction de la longitude moyenne, on représente par $\frac{a}{a'} b_{101}$ le coefficient de *l'inégalité parallactique* ou de l'inégalité qui a pour argument la longitude moyenne du Soleil moins celle de la Lune, on aura :—

$$b_{101} = \frac{15}{8} m + \frac{93}{8} m^2 + \frac{6887}{128} m^3 + \frac{137197}{512} m^4$$

$$+ \frac{4630061}{3072} m^5 + \frac{220740555}{24576} m^6$$

$$+ \frac{15}{2} m e^2 + \frac{15}{8} m e'^2 - \frac{165}{32} m \gamma^2.$$

Si l'on réduit en nombres les différentes termes de cette

* See vol. xxiii. pp. 210 and 242.— Ed.

expression,* qu'on les multiplie ensuite par le rapport $\frac{a}{a'}$ du demi-grand axe de l'orbe lunaire au demi-grand axe de l'orbe du soleil, en supposant comme nous l'avons fait dans la théorie de la Lune

$$\frac{a}{a'} = 0{\cdot}00252551,$$

on trouvera

$$\frac{a}{a'} b_{101} = 73''{\cdot}061\ (3) \quad + 33''{\cdot}883\ (4) \quad + 11''{\cdot}012\ (5)$$

$$+ 4''{\cdot}370\ (6) \quad + 1''{\cdot}839\ (7) \quad - 0''{\cdot}820\ (8) \quad - 0''{\cdot}3\ (\text{indet.})$$

$$= 125''{\cdot}285.$$

M. Airy par une discussion approfondie des observations a trouvé pour ce coefficient $124''{\cdot}7$ ou $125''{\cdot}5$, en faisant usage des seules observations altazimuthales qui selon lui méritent le plus de confiance. Il suffirait donc d'une très légère altération dans la valeur que nous avons adoptée pour le rapport $\frac{a}{a'}$ pour faire coincider *identiquement* les résultats de la théorie et de l'observation ; ainsi en supposant par exemple

$$\frac{a}{a'} = 0{\cdot}00252985,$$

on trouvera

$$\frac{a}{a'} b_{101} = 125''{\cdot}5$$

c'est à dire la valeur donnée par le savant directeur de Green— [illegible]n-wich comme la plus probable. Nous avons supposé dans notre [illegible]tre théorie la constante de la parallaxe égale à $3421''{\cdot}153$; si l'on [illegible]on multiplie cette constante par la valeur précédente du rapport [illegible]ort $\frac{a}{a'}$, on trouve

$$8''{\cdot}655$$

pour la parallaxe du Soleil à l'équateur, valeur qui s'accord [illegible]e avec celle qu'on a déduite des derniers passages de *Venus* sur l[illegible]e disque solaire.

* M. Plana a donné pour le même coefficient une expression qui diffè[illegible]re légèrement de la précédente à partir du terme de l'ordre m^3, mais cela tien[illegible] à quelques incorrections qui se sont glissées dans son calcul, et qui n'ont [illegible]du reste aucune influence sensible sur les résultats numériques.

La théorie se trouve donc ainsi par rapport à *l'équation parallactique* en parfait accord avec l'observation; mais cela suppose qu'on n'a point égard à la correction résultante de la réaction de la Lune sur la Terre, correction qui consiste à multiplier tous les termes dépendants du rapport $\frac{a}{a'}$ par le coefficient $\frac{1-\nu}{1+\nu}$, où ν représente le rapport des masses de la Lune et de la Terre; ce coefficient, qui réduit en nombres serait de $\frac{37}{38}$ d'après les suppositions les plus probables sur la masse de la Lune, diminuerait de $\frac{1}{38}$ la valeur précédente du coefficient $\frac{a}{a'} b_{101}$, ainsi on aurait

$$\frac{a}{a'} b_{101} (1 - 2\nu) = 121''\cdot988,$$

et il serait impossible de faire accorder ce coefficient avec celui déduit des observations par M. Airy, à moins de supposer à la parallaxe solaire une valeur beaucoup plus forte que celle qu'on a conclue des autres moyens que nous avons de la déterminer, et qui paraîtrait inadmissible au moins pour le moment. Il faudra donc, si l'on veut accorder aux résultats des recherches de M. Airy une entière confiance, comme elles le méritent, rejeter tout-à-fait l'emploi de ce coefficient, et alors on rétablira par rapport à *l'inégalité parallactique* comme cela a lieu relativement aux autres inégalités du mouvement lunaire, *un accord presque complet* entre les résultats de la théorie et ceux de l'observation.

Quant à la valeur de la parallaxe solaire déduite des tables lunaires de M. Hansen, question soulevée par M. Stone, et à laquelle l'auteur a répondu dans les *Monthly Notices* (vol. xxiii. p. 242), nous remarquerons que M. Hansen donne pour le coefficient de l'équation parallactique déduit de ses calculs $121''\cdot368$, évaluation évidemment beaucoup trop faible s'il n'a pas pris en considération la correction provenant de la réaction de la Terre sur la Lune. Il multiplie, comme à son ordinaire, ce coefficient par le facteur empirique 1·03573 pour rapprocher sa valeur de l'observation, et il trouve alors $125''\cdot70$ pour le coefficient de l'équation parallactique, valeur très approchée de celle que M. Airy a déduite des observations altazimuthales; mais si au lieu du coefficient empirique 1·03573 M. Hansen eut employé au contraire le facteur 0·97369 qui résulte d'une théorie rigoureuse, il est évident qu'il se serait considérablement éloigné de l'observation au lieu de s'en rapprocher. Enfin M. Hansen, en multipliant par ce même facteur 1·03573 la parallaxe solaire qu'il avait trouvée de $8''\cdot66$ conformement à toutes les notions reçues, en conclut que cette parallaxe dont s'élever à $8''\cdot97$, résultat qu'il donne comme la véritable conclusion à tirer de la discussion de ses nouvelles tables lunaires, et qui diffère très peu, ajoute-t-il, de la valeur

qui a été déterminée par la dernière opposition de *Mars*. Mais il est impossible d'appuyer aucune opinion certaine dans une question aussi délicate, sur de pareilles déductions dépendantes se l'emploi d'un coefficient tout-à-fait arbitraire, et dont ni l'observation ni la théorie n'ont pu jusqu'ici justifier l'introduction dans la théorie de la Lune. En effet il a été démontré,* que les formules de M. Plana et celles que j'ai données dans le vol. iv. de la *Théorie Analytique* représentent les mouvements de la Lune *au moins aussi exactement* que celles de M. Hansen, et elles ont sur elles l'avantage de reposer sur une analyse rigoureuse, d'être dégagées de tout empirisme, et de ne demander à l'observation que les élémens qu'il est indispensable de lui emprunter.

(*Note.*—The factor $\frac{1-\gamma}{1+\gamma}$ is given, in its symbolical form, in M. de Pontécoulant's *Théorie Analytique*, vol. iv. p. 547; and is introduced in the numerical computation of the coefficients of terms for the Moon's longitude, p. 603, giving the value 122·378. The same factor, in the form $\frac{E-M}{E+M}$, is found in the *Mathematical Tracts, Lunar Theory*, by the simple and accurate method of commencing the lunar investigation by referring the Sun's elliptical co-ordinates to the centre of gravity of the system of the Earth and Moon.—G. B. A.)

Proper Motions of the Stars of the Greenwich Seven-Year Catalogue of 2022 *Stars for* 1860, *not included in the Greenwich Twelve and Six-Year Catalogues, deduced by Comparison with the Results of Bradley's Observations, as given in Bessel's Fundamenta Astronomiæ.* By E. J. Stone, Esq.—(Abstract.)

The Memoir now presented to the Society may be considered a continuation of the Memoirs of Mr. Main, printed in the xixth and xxviiith volumes of the Society. It contains the proper motions of 459 stars.

The constants adopted are those of Professor Peters (*Numerus Constans Nutationis*).

A considerable number of the stars whose proper motions are given in the present paper are within a few degrees of the pole, and the interval between the two Catalogues compared is considerable, viz. 105 years. I have therefore used the trigonometrical formulæ for bringing up Bradley's observations to the beginning of 1860.

Greenwich, Dec. 23*d*, 1863.

* Voir *The Theory of the Moon*, by Sir John Lubbock, Bart., part x.

Note on the Variable Nebula in Taurus, R. A. (1860) $4^h\ 13^m\ 47^s{\cdot}6$; N.P.D. 70° 48′ 41″. By J. R. Hind, Esq.

The night of the 12th of December last was one of the finest for telescopic observation here that I remember to have witnessed. It was on a similarly advantageous night (that of October 11, 1852) when the above nebula was discovered at Mr. Bishop's former observatory in the Regent's Park, and when it could not be overlooked with a low power on his 7-inch refractor. I have long been anxious, since the variability was detected, to examine the vicinity under the like favourable conditions, but, from one cause or another, have never succeeded until the above-mentioned date. On applying the same powers with which I saw it readily in 1852, and repeatedly distinguished it afterwards, when the sky was not so clear, I was unable to perceive the least trace of it: the star which formerly appeared almost to touch the nebula at its north-following limit, and which is also variable, was a 9·6 magnitude in Argelander's scale, or about as bright as in October 1852, but with the optical means at my command there was an entire absence of nebulosity in its neighbourhood. It is evidently an object deserving of continued attention at the hands of observers who are provided with instruments of great power.

Mr. Talmage, whose sight is remarkably acute, and who, on the night in question, could distinguish eleven stars in the *Pleiades*, was equally unsuccessful with myself in discovering any trace of the nebulosity.

Mr. Bishop's Observatory, Twickenham,
Jan. 7th, 1864.

Mr. Carrington has had his attention called to a passage in an old black-letter book, entitled "The Pageant of Popes, written in Latin by Maister Bale, and now Englished with sundrye additions by J. S., Anno 1574," which, he thinks, may deserve republication. (J. S. is John Studley.)

Folio 52.—"Vspergensis saith, that, in this time of Leo, the Sunne was darkened and lost his light for eightene days, so that the shippes ofte on the sea wandred to and fro: Also that in an other yeare it was twyse in the Eclipse: firste in June, secondly, in December: Likewise the same yere, the Moone was twyse in the Eclipse, in July and in January."

Leo the Third, here referred to, became Pope in 796, and died in 816.

The above occurrence is not among the recorded darkenings of the Sun collected by Baron von Humboldt. R. C. C.

Errata in Shortrede's Tables.

Major-General Shortrede has communicated the following list of Errata:—

Preface, page	v.	line 1		*for*	(8)	*read*	(9)	S.
	vii.	form (6)			$\left(\frac{120}{6\pi^6}\right)$		$\left(\frac{720}{6\pi^6}\right)$	S.
Tab. Log.	13	Num.	6844	*for*	3·831	*read*	3·835	S.
	117	Antilog. to	·06459	*for*	0358	*read*	0353	
	"		·06460		0380		0379	
	120		·09967		7970		7969	
	125		·15677		4629		4729	
	127		·18140		8488		8448	S.
	"		·18299		4028		4018	
	128		·19954		3214		3215	
	129		·20816		4955		4953	
	133		·26374		5449		5439	
	135		·28079		89		8930	
	137		·30488		78		7809	
	139		·33338		4656		4666	
	"		· 339		4706		4716	
	147		·42019		1413		1419	
	"		·43161		1551		1531	
	163		·62163		4399		4369	
	167		·66088		0151		0153	
	168		·67461		7286		7266	
	"		·68088		6008		6009	
	174		·75472		4963		4863	
	177		·78737		8923		8723	
	"		·79089		8509		8599	
	188		·91679		6396		6386	

Excepting the one on p. 127, marked S., all the others in the Antilogarit[illegible] have been copied from Dodson.

November 13, 1863.

RECENT PUBLICATIONS.

Bonner Sternverzeichniss dritte Section, enthaltend die genäherten mittleren Oerter von 108129 *Sternen zwischen* 41 *und* 90 *Grad nordlicher Declination fur den Anfang des Jahres* 1855. *Unter Mitwerkung der Herren Professor Dr. E. Schonfeld, und Professor Dr. A. Krüger, auf der Sternwarte der Königlichen Rheinischen Friedrich-Wilhelms-Universität zu Bonn beobachtet und berechnet von Dr. F. A. Argelander, Director der Sternwarte.* Bonn, 1862. (Being Vol. V. of the Astronomical Observations of the Observatory.)

The Introduction to this, the concluding volume of Dr. Argelander's Survey of the Northern Heavens, contains some interesting statistical notices of the whole undertaking. His co-labourers in the observations were, from the beginning until the 5th May, 1853, Herr F. Thormann; since 28th Feb. 1853, Professor Schönfeld; and since the 28th Aug. 1853, Professor Krüger. Professor J. Schmidt also assisted; but the success of the undertaking is in a great measure due to Professors Schönfeld and Krüger, whose zealous co-operation is warmly acknowledged.

After some preparatory observations by way of practice, the first of the comet-seeker zones used for the Catalogue was observed the 25th Feb. 1852, the last on the 27th March, 1859; and these observations, the foundation of the whole work, thus lasted seven years and one month; in which period, during 625 nights, 1841 zones were observed, as follows

1852	...	...	82	zones in	50	nights.
1853	...	...	219	,,	83	,,
1854	...	...	285	,,	91	,,
1855	...	...	199	,,	75	,,
1856	...	...	347	,,	99	,,
1857	...	...	335	,,	106	,,
1858	...	...	311	,,	99	,,
1859	...	...	63	,,	22	,,

viz. 12 zones Argelander, 110 Thormann, 909 Schönfeld. On an average, 3 zones were observed in each night; but when the whole night was clear, and without moonshine, and after Schönfeld's entrance, when two observers could be employed alternately, usually 5 or 6, not seldom 7 or 8, and on two occasions 9 zones, occupying one time 12 hours and the other time 10½ hours. The month of October was the most favourable.

In the seven October months, in 71 nights, 257 zones were observed, being on the average 3·6 zones per night; but in October 1856, in 15 nights, 72 zones were observed. This was the most favourable month; next to it comes March, 1856, when, in 13 nights, 61 zones were observed. But of the 1841 zones, 44 were more southerly than $-2°$, and so were not used for the present Catalogue, which thus depends upon 1797 zones. These include about 850,000 single observations, or on the average, 473 to each zone, or, reckoning one hour to each zone, about 8 observations per minute; but in the Milky Way the number of observations in the hour was from 900 to 1000; and frequently, in the constellation *Cygnus*, and twice in *Orion*, even the latter number was exceeded. The highest number was 1226 in the hour, or nearly 20½ per minute. The revision zones were observed only by moonlight, or rarely by twilight, chiefly in order not to be confused by a multitude of small stars, and so to be able to better estimate the magnitudes. The telescope employed was, at first, a Fraunhofer of 4½-feet focus, but afterwards one of 5-feet focus. The 44th revision zone was the first one so observed.

Between 1854, Jan. 19, and 1861, April 19, there were observed 476 revision zones, 172 by Schönfeld and 304 by Krüger; the whole number of nights being 227, viz.

1854	...	...	31	zones in	22	nights.
1855	...	...	28	,,	15	,,
1856	...	...	61	,,	36	,,
1857	...	...	46	,,	24	,,
1858	...	...	126	,,	47	,,
1859	...	...	127	,,	50	,,
1860	...	...	53	,,	30	,,
1861	...	...	4	,,	3	,,

the month of October being for these observations also the most favourable, viz. in 33 nights 72 zones. The total number of observations in the 476 zones was about 137,000, or 287 per zone. The entire number of observations for the formation of the Catalogue is estimated at 1,061,000, or considerably over a million. After some slight corrections — indicated in the preceding volumes, or at the end of the Introduction to the present volume — the numbers of stars in the three sections of the Catalogue respectively are 110,987, 105,082, and 108,129; making together 324,198. Some interesting remarks are made as to the distribution of these stars over the heavens, and in relation to the Milky Way.

Parallaxen-Bestimmungen mit dem Königsberger Heliometer. Von Herrn Dr. A. Auwers, *Ast. Nach.*, Nos. 1411–1416.

This elaborate series of researches, founded on observations in the years 1860–62, relates to the Relative Parallaxes of 1°, 61 *Cygni* in comparison with two neighbouring stars; 2°, 21258 Lalande in comparison with two neighbouring stars; 3°, *ζ Ursæ Majoris* in comparison with *Alcor*; 4°, *Procyon* in comparison with a neighbouring telescopic star. The results obtained seem to be that the Relative Parallax of *ζ Ursæ Majoris* and *Alcor* is insensible; that the Parallax of *Procyon* is sensible, and probably between 0″·15 and 0″·35, while for the other two cases the results are given as follows,

21258 Lalande,—

Relative Parallax = 0″·2622, Mean Error ± 0″·0161, Prob. Error ± 0″·0109

which on the hypothesis in Peters' *Recherches sur la Parallaxe* &c., as to the probable parallax of the small stars according to their magnitudes, gives for the most probable value of the Absolute Parallax of 21258 Lalande,

$$0''\cdot2709 \pm 0''\cdot0112$$

corresponding to a distance of 761000 ± 3200 times that of the Sun = 12·01 ± 0·50 light-years.

61 *Cygni*,—

Relative Parallax = 0″·2588, Mean Error ± 0″·0238, Prob. Error ± 0″·0161

which on the same hypothesis above referred to gives for the most probable value of the Absolute Parallax of 61 *Cygni*,—

$$0''\cdot5638 \pm 0''\cdot0162$$

corresponding to a distance of 366000 ± 11000 times that of the Sun, or 5·77 ± 0·17 light-years.

The last-mentioned result agrees very closely with M. Otto Struve's value of the Parallax of 61 *Cygni*, viz.,

$$0''\cdot5060 \pm 0''\cdot0282$$

for the Relative Parallax, and

$$0''\cdot5107 \pm 0''\cdot0283$$

for the Absolute Parallax; the unexpected magnitude of which gave occasion to M. Auwers' researches on this star.

Instrument for Sale.

A fine Refractor by T. Cooke and Sons, York; 4 inches clear aperture, $4\frac{1}{2}$ feet focal length; four Astronomical Eye-pieces of 35, 70, 120, and 230 powers respectively; Terrestrial Eye-piece with 60 power; Sun Prism, or first-surface glass reflector; Walnut Stand on three legs (portable), carrying an equatoreal mounting, to follow a star, by a handle and endless screw. The whole packs in a deal case. May be seen and tested. Price 40*l.*—Edmund Wheeler, 48 Tollington Road, Holloway, London, N.

ERRATA.

In Monthly Notices, No. II.

Page 38, line 11 from bottom, *for* curves, *read* sum.
— 41, in list of Chinese Eclipses, No. 10, *dele* Kwei Hae and April 15.
— 42, line 10, No. 22, *for* 542, *read* 552.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIV. *Feb.* 12, 1864. No. 4.

THE Annual General Meeting of the Society, Rev. R. MAIN, Vice-President, in the Chair.

G. F. Chambers, Esq., 2 Palace Gardens, Kensington;
J. B. N. Hennessey, Esq., Indian Trigonometrical Survey;
R. Webster, Esq., 74 Cornhill;
Alfred Cooper, Esq., Training College, Highbury;
Henry Roe, Esq., Training College, Highbury; and
Capt. J. Higgens, 38 Harpur Street, Bedford,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Forty-fourth Annual General Meeting of the Society.

In former years the Financial and Statistical Statements were brought up to Feb. 8 in each year: then, when the interval between that day and the day fixed for the Anniversary Meeting was found to be inconveniently short, a mixed system was fallen into of closing the Financial Account at Dec. 31 preceding, and the Statistical Statement on Feb. 8. Inconveniences having thereby arisen, the Council have resolved for the present and future years to present both statements made up to Dec. 31 of the year preceding; and taking into account the expansion of the Society's affairs since its practice in these respects was last considered, the Council have further resolved to present the accounts of the year by Balance-Sheet and Profit-and-Loss Account, in place of the former Cash Account of the Treasurer, which has been audited as usual. On this occasion a Balance-Sheet is given for the commencement as well as for the end of the year

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patronesses, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 8, 1863...	163	245	31	5	444	47	491
Since elected	4	9	...	...	...	4	...
Deceased	−4	−3	−8	−1	...	−4	...
Removals	1	−2	1	...	...	...	...
Resigned	...	−5	...	...	...	...	...
Replaced	...	...	...	1	...	...	...
Dec. 31, 1863	164	244	24	5	437	47	484

BALANCE SHEET, DECEMBER 31, 1862.

Dr. £	s.	d.					Cr. £	s.	d.
–	–	–	Cash in hand.						
240	8	8	Banker's balance.						
			Proceeds of Turnor fund	...	...		69	16	10
			Proceeds of Lee fund	...	...	...	11	9	3
			Due to Mrs. Jackson	...	...	...	–	–	–
390	14	0	Annual contributions—arrears.						
			,, paid in advance				6	6	0
2	2	0	Admission fee—1 due		...	...			
2	1	8	Due for publications.						
			Reserve against sums due		...	...	40	0	0
2100	0	0	3 per Cent Consols at par.						
			,, Turnor fund		...	...	500	0	0
			,, Lee fund		...	...	110	0	0
3900	0	0	New 3 per Cents at par.						
			,, Jackson fund		...	...	300	0	0
			Sundry debts unpaid	...	...	...	44	0	0
31	10	0	3 medals in stock.						
–	–	–	Unsold publications.						
–	–	–	Books and instruments.						
			Composition fund	...	£3381	0 0			
			Net capital	...	...	2204 4 3	5585	4	3
6666	16	4					6666	16	4

PROFIT AND LOSS ACCOUNT.

	£	s.	d.	£	s.	d.
				Cr.		
By New compositions	...		...	147	0	0
Compositions set free	...		...	84	0	0
Admission fees	...		...	35	14	0
First year's contributions	...		...	22	1	0
Annual contributions	...		...	501	18	0
Dividends, January	30	6	5			
,, April	56	6	2			
,, July	30	11	8			
,, October	58	5	0	175	9	3
Sale of publications	...		...	73	6	2
By Investing under par	...		...	26	3	2
				1065	11	7
				Dr.		
	£	s.	d.	£	s.	d.
To Composition fund	...		...	147	0	0
Turnor fund	15	0	0			
Lee fund	3	5	6			
Mrs. Jackson	8	13	4	26	18	10
Rent	...		...	–	–	–
Editor	60	0	0			
Printing	257	18	6			
Engraving and lithographing ...	62	3	9			
Binding	7	4	9			
Assistant-Secretary	100	0	0			
Collection	31	14	0	519	1	0
Taxes	6	3	5			
Postage	33	2	0			
Parcels	5	1	7			
House Expenses	24	10	0			
Coals and gas	17	7	6			
Meetings	17	10	0			
Insurance	12	5	6			
Repairs	5	8	6			
Medal (none awarded)	–	–	–			
Sundries	19	0	3	140	8	9
Sums written off	14	14	0			
Reserve fund	25	0	0			
To Capital—increase	192	9	0	232	3	0
				£1065	11	7

BALANCE SHEET, DECEMBER 31, 1863.

Dr. £	s.	d.				Cr. £	s.	d.
–	–	–	Cash in hand.					
353	4	1	Banker's balance.					
			Proceeds of Turnor fund	...	...	82	2	10
			Proceeds of Lee fund	...	...	14	14	9
			Due to Mrs. Jackson	...	...	–	–	–
376	0	0	Annual contributions—arrears.					
			,, paid in advance			4	4	0
3	0	8	Due for publications.					
			Reserve against sums due	...	...	65	0	0
2100	0	0	3 per Cent Consols at par.					
			,, Turnor fund	...	...	500	0	0
			,, Lee fund	...	...	110	0	0
4218	5	8	New 3 per Cents at par.					
			,, Jackson fund		...	300	0	0
			Sundry debts unpaid	...	...	165	5	7
31	10	0	3 medals in stock.					
–	–	–	Unsold publications.					
–	–	–	Books and instruments.					
			Composition fund ...	£3444	0 0			
			Net capital	2396	13 3	5840	13	3
7082	0	5				£7082	0	5

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	20	X.	191	XXI. Part 2 (separate).	100
I. Part 2	62	XI.	201	XXI. (together).	107
II. Part 1	79	XII.	207	XXII.	199
II. Part 2	43	XIII.	221	XXIII.	201
III. Part 1	96	XIV.	408	XXIV.	202
III. Part 2	116	XV.	190	XXV.	216
IV. Part 1	117	XVI.	213	XXVI.	225
IV. Part 2	129	XVII.	191	XXVII.	485
V.	143	XVIII.	191	XXVIII.	442
VI.	164	XIX.	205	XXIX.	472
VII.	188	XX.	198	XXX.	239
VIII.	174	XXI. Part 1 (separate).	316	XXXI.	255
IX.	178				

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{1}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed, repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and 2¼-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. 2½-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Lee* circle, to Mr. Burr.
The *Beaufoy* clock, to Dr. Booth.
The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument, No. 1, to Mr. Lassell.
Ditto ditto No. 2, to Mr. De La Rue.
Ditto ditto No. 3, to Rev. C. Pritchard.
Ditto ditto No. 4, to Rev. F. Howlett.
Ditto ditto No. 5, to Mr. Birt.
Ditto ditto No. 6, to Rev. J. Cape.
Ditto ditto No. 9, to Mr. Lockyer.
Ditto ditto No. 19, to Mr. Dayman.
Ditto ditto No. 25, to Mr. Dayman.
Ditto ditto No. 41, to Rev. C. Pritchard.
Ditto ditto No. 43, to Mr. Huggins.
The 6-inch circular protractor, to Mr. Birt.

The *Beaufoy* clock, lent to the Royal Society on July 12, 1839, has at length been traced and returned to the Society.

The Volume of Memoirs.

The principal topics discussed in Vol. XXXII. of the *Memoirs* are embodied in the account given of the progress of Astronomy during the past year; it is, therefore, unnecessary here to open any formal remarks on their several contents. Nevertheless, the Council will be justified in tendering the thanks of the Society at large to the Astronomer Royal, for his kindness in procuring from the Admiralty the gratuitous insertion of his interesting Memoir on the Determination of the Longitude of Valencia. The readers of this paper cannot fail to be struck with the almost unexpected exactness of the coincidence of the longitude, as obtained by the transmission of chronometers with the value of the same element, as obtained by means of a long electric wire. It is curious to observe herein, how, as in a thousand other instances, no sooner is one method of operation brought to perfection, than it is doomed to be supplanted by some other process of readier or more accurate application.

The readers of the volume will also be gratified with the careful delineation of the features of *Mars*, as seen telescopically at his last opposition by Lord Rosse, Mr. Lassell, and especially by Mr. Lockyer. In due time these pictures cannot fail to assist in the elimination of the appearances due to the planet's atmosphere from those which belong to the physical conformation of the surface. Mr. Main's measures of the ellipticity and dimensions of the same planet, independently of their intrinsic value, are an evidence that he will not permit the Oxford heliometer to remain idle under his keeping.

Obituary.

The Society has to regret the loss by death of the following Fellows and Associates:—

Honorary.—Baron von Senftenberg.

Fellows.—Rev. J. E. Ashby.
Beriah Botfield, Esq.
Josh. Gwilt, Esq.
T. C. Janson, Esq.
J. Jesse, Esq.
Commander M. Montagu.
Admiral Washington.
Rev. Thos. Belgrave, *Non-Resident.*
Rev. Jas. Grooby. „
Dr. Haldane. „
Jno. Fardell, Esq. „
Jas. Jardine, Esq. „
Commander Molesworth. „
Josh. Moore, Esq. „
Rev. W. Potchet. „

Associates.—Prof. Amici.
M. Capocci.
M. Mosotti.
M. le Baron Plana.
M. Weisse.

The King of Siam, an Honorary Member, reported as dead, is still alive.

John Eyre Ashby was born on the 22d January, 1820. He was educated at University College School, London, where he obtained several distinctions. and in 1840 graduated B.A. in the University of London. After several years in Homerton College, devoted more especially to his training for the work of a Congregational Minister, he laboured both as a Pastor and as a Lecturer on the higher Mathematics, successively in Arundel, Brighton, and London. In 1856 he established a boys' school at Enfield, and carried it on with success till his premature death on December 22d, 1863. In 1858 he proceeded with distinction to the degree of LL.D. in the University of Jena. He was elected a Fellow of the Royal Astronomical Society in 1843.

Of Dr. Ashby's kindness and courteousness,—of his genuine wit and sense of humour,—of his industry and energy,—of the liberality of his religious convictions,—of the wide range of his acquirements, and the strong grasp of his mind,—many men in the extensive circle of his acquaintance and friends can testify. Taken so early from the sphere of his activities here,

before a time of leisure had arrived, Dr. Ashby had not completed much original work. His early devotion to Mathematics and Astronomy gave way to an increasing fondness for Chemistry and Chemical Physics. On these and kindred subjects he gave interesting and well-illustrated lectures, not only to his own school, but to various public institutions.

BERIAH BOTFIELD was born in the year 1807. He was educated at Harrow, the school of which he subsequently endowed with the "Botfield Medal for Modern Languages." From Harrow he proceeded to Christ Church, Oxford, and took his degree of B.A. in 1828.

Mr. Botfield was principally distinguished as a bibliographer, in which pursuit he was almost unrivalled. He published several works on bibliographical and antiquarian subjects, and communicated numerous papers on similar subjects to the *Gentleman's Magazine* and other periodicals. In 1840 he was returned to Parliament for Ludlow, and although at a subsequent election in 1847 he was unsuccessful, he regained his seat in 1857, and sat for that borough from that time to the close of his life.

His library at Norton Hall, Northamptonshire, is chiefly remarkable for a fine collection of *editiones principes* of classical authors, and costly folio editions of illustrated works.

He was a Fellow of the Royal Society, and of the Society of Antiquaries, and joined this Society in 1839. He died on the 7th of August, 1863, at the age of 56 years.

JOSEPH GWILT was born in the parish of St. George the Martyr, Southwark, January 11, 1784. He was the son of Mr. George Gwilt, architect, which profession he also followed, and at his decease was looked upon as one of its most eminent members. He was the author of many valuable works, chiefly relating to architectural literature. The chief of these is his *Architectural Dictionary*, which is a production of extraordinary merit, whether for the purposes of elementary and advanced study, or for those of general reference. It has gone through many editions, and is much valued by the profession. In 1815 he became a Fellow of the Society of Antiquaries, and in 1833 was elected a Fellow of this Society. He died on the 14th of September, 1863, in his eightieth year.

THOMAS CORBYN JANSON was born on the 1st of July, 1809. He was educated by Dr. Morell, of Hove, near Brighton, and acquired great proficiency in the learned languages. In his early youth he was much distinguished for his taste for natural history, which continued his favourite pursuit during his life. He also paid considerable attention to astronomy, and was a careful and laborious observer of transits, occultations, and eclipses. In 1860, while travelling in the south of France, he

crossed the Spanish frontier in order to observe the Total Eclipse of the Sun which occurred in that year. The place he selected was Burgos, where he had an unclouded view of the eclipse. The observations he made on that occasion appeared in one of the periodicals of the day, and also formed the subject of a paper read to this Society. He was for many years a Fellow of the Linnean Society, where he was much respected as a sound and well-informed naturalist. He joined this Society in 1848, and frequently attended the meetings, but, with the exception of the paper relating to the Eclipse, never communicated any results of his astronomical observations. He died June 23, 1863, in the fifty-fourth year of his age. In his will he left the sum of two hundred pounds, free of legacy duty, payable after the decease of his widow, to be added to the fund in connexion with the Royal Astronomical Society, called "The Lee Fund."

Admiral WASHINGTON was born on the first day of the present century. He entered the Navy on May 15th, 1812, as a first-class volunteer on board the "Junon," 46 guns, Captain James Sanders, fitting for the North American station, where he took part in many operations in the River Chesapeake. He assisted in making prizes of several of the enemy's vessels, and contributed to the complete discomfiture of fifteen gun-boats that had been despatched for the express purpose of capturing the "Junon," after an action of three hours, fought on June 20, 1813. Removing as midshipman in the following October to the "Sybille," he sailed in that ship in 1814, under Captain Forrest, with the "Princess Caroline," Captain Downman, for the latitude of Greenland, in fruitless pursuit of the American Commodore Rogers. In November of the same year, having returned to England, he entered the Royal Naval College at Portsmouth, and this unquestionably was the turning-point in his career. Naturally of studious habits, he turned to the greatest advantage the opportunity afforded him of improving his education; and the mental training he thus received enabled him in subsequent years to improve the talents he possessed, not in the accumulation of wealth, but in advancing the interests of his profession, and in helping forward the cause of humanity. On leaving the Royal Naval College, he was received in May, 1816, on board the "Forth," Captain Sir Thomas Louis, under whom he was again employed for upwards of three years on the coast of North America. He then in succession joined the "Vengeur" and the "Superbe," both on the South American Station, where he remained until after his promotion to the rank of Lieutenant, which took place on the 1st January, 1821. He was subsequently employed on particular service, and in August, 1830, was appointed to the "Royal George," 120, as Flag-lieutenant to Admiral St. John Poer Beresford, Bart., Com-

mander-in-Chief at the Nore—continuing to serve under that officer, in the "Ocean," until advanced to the rank of Commander in 1833.

To the active service consequent upon his various appointments he had united the practice of maritime surveying, and the combined pursuits of a scientific hydrographer and geographer. In 1835 he succeeded Captain Maconochie as Secretary of the Royal Geographical Society of London, but resigned that office in 1841, on being appointed to continue the survey of the North Sea, which had for some time been in progress. During this undertaking, in which he was continually engaged until the close of 1844, he was necessarily occupied in correcting the existing charts, as the positions of the shoals and the directions of the navigable channels had in many cases become changed. This service was strikingly useful, and led in a great degree to his subsequent appointment as a Royal Commissioner on certain important questions respecting the construction of harbours of refuge on exposed coasts of the country.

In 1842 he was appointed to the rank of Post-Captain, in compliment to the King of Prussia. This survey was Captain Washington's last service afloat. In 1845 he was appointed a member of a Royal Commission for inquiring into the state of the rivers, shores, and harbours of the United Kingdom, the duties of which office brought him into contact with a large number of our seamen and fishermen, in whose welfare he took unceasing interest from this period until the day of his death.

He was subsequently engaged in an inquiry into the condition of our large fisheries on the north-east coast of Scotland. His able Report, and the clear plans of the different classes of fishing-boats which accompanied it, prepared expressly by Mr. James Peake, Master Shipwright of Her Majesty's Dockyard, Devonport, deservedly attracted considerable attention.

"When we remember," said the late Admiral, "that the fishing-boats of the United Kingdom number probably 36,000, manned by 150,000 men and boys, it will be admitted that this class of men deserves some consideration, and that, as far as may be, their small harbours should be deepened, to enable them to obtain shelter in time of need, and at all states of the tide."

In 1853 Captain Washington visited some of the Russian fortresses in the Baltic. In the following year the war broke out, and the results of his acute observations during that tour proved of the greatest value. He was thus enabled to give most important information regarding the character of those fortresses, which could not possibly be obtained from any other recent and reliable source. His experience was afterwards turned to good account by the assistance he continued to render, during the whole period the war lasted, to the Admiralty.

In the year 1855 he was appointed by Sir James Graham, Bart., M.P., then first Lord of the Admiralty, to the responsible office of Hydrographer of the Admiralty on the retirement and special recommendation of the late Admiral Sir Francis Beaufort, F.R.S., to whom he was much attached. With reference to this appointment, Sir James stated, in the House of Commons, at the close of the Russian war in 1856, that during his long official life no appointment made by him had afforded him more satisfaction than that of Admiral Washington. To the responsible duties of that office he devoted himself with such assiduity, that his valuable life was unquestionably brought to a premature end by over-mental exertion in connexion with it.

"When first appointed to the surveying service," writes a gentleman who had served under Admiral Washington in that department, "no officer could have carried on the duties with more zeal, activity, and intelligence. His zeal and abilities were conspicuously shown forth by the mass of information he published to the world during the few years he was at the head of the Hydrographic Office, in Charts, Sailing-directions, Tide-tables, &c. Indeed, he had not held that appointment above three or four years before he had arranged for publication the results of numerous nautical surveys and other useful professional information which had accumulated in the office. His quickness and clearness of judgment in revising charts and other office works were conspicuous to every one, and the amount of work he undertook and accomplished showed his great application and abilities."

In 1858 Captain Washington again became a Member of the Royal Commission to inquire into the proper sites for Harbours of Refuge along the coasts of the United Kingdom. We are informed that the remarkable extent of his local knowledge of the districts visited by this Commission, and his active and painstaking habits, were of great value to the Commission, indeed, that it was looked upon by him as one of the crowning efforts of his labours in the cause of humanity, and on behalf of his humbler professional brethren.

Thus he continued to discharge, without intermission, the onerous duties of his office. Returning in the evening to his own home, these duties were occasionally pursued until the early hours of the morning. He was thoroughly devoted to his profession, and especially to all matters connected with hydrographic science; and no labour, no trial, was too great for him, so long as his public duties were satisfactorily discharged.

In the year 1862 Captain Washington was promoted to the rank of Rear-Admiral, and in the same year one of his last public labours was to act as a Juror at the International Exhibition of 1862. He was unanimously elected Chairman of [illegible] the subjects he had so long studied.

His funeral took place at the Protestant cemetery of St. Marie, Havre, on the 19th September, with every demonstration of respect on the part of the foreign authorities of the town. Admiral Washington's character was much valued in France; and in consequence, immediately his decease became known, the heads of departments at Havre—civil, naval, and military—expressed their desire to attend the funeral. Officers and men of the Imperial yacht "Prince Jerome," to the number of forty, formed part of the *cortège*. The English ships in the harbour hoisted their colours half-mast high, and the captains of two large steamers volunteering the attendance of six of their seamen as bearers, the offer was gratefully accepted by the family. The French authorities all attended in full uniform; and the inhabitants of Havre, thronging the streets, silently and reverently testified their respect and regret. The naval Aide-de-Camp to the English Embassy in Paris, and another English naval officer, were also present. The unexpected sympathy shown by the French in thus honouring the Admiral's memory was gratefully felt and acknowledged as a consolation by his afflicted family; and the Lords Commissioners of the Admiralty afterwards expressed officially to the authorities of Havre their deep sense of the honours so gracefully bestowed.

Thus closed the life of one of the most valuable officers in Her Majesty's Service. That his years should have been cut in the midst of an active professional life must be to Admiral Washington's friends an occasion of regret; but they may, however, feel grateful that so much has been accomplished by him.

Jean Baptiste Amici was born on March 23, 1786, at Modena, where he studied the higher branches of mathematics under Ruffini, and while yet extremely young was appointed Professor in the University of his native town. In 1831 he was invited to Florence, and nominated to the direction of the Observatory of that city, as successor to the celebrated astronomer Pons. Amici remained at Florence until the day of his sudden death, on April 10th, 1863.

Amici was the inventor and constructor of many ingenious contrivances in connexion with both the microscope and the telescope. His pretty adaptation of four plane prisms to the formation of an achromatic telescope will always be read with pleasure, and associate his name among men famous for optical skill.

At a time when an effective achromatic objective was beyond the reach of hope for the microscope, Amici invented and brought to perfection a reflecting instrument, which is in itself no mean rival of that noble form to which opticians have at length brought the microscopic object-glass in our own days. It is said that some of Amici's microscopes possessed a

magnifying power of one thousand linear, with sufficient light and distinct definition. In more recent times Amici devoted much attention to the construction of achromatic objectives for the microscope, and in that particular form, when much loss of light by reflection is obviated by the immersion of the posterior lens in a fluid, he was probably equal to the best of our justly celebrated English constructors.

Amici, among other interesting novelties in optical instruments, devised a small and very portable level for agricultural purposes, adapted to a short telescope resembling an opera glass, but which nevertheless was provided with wires of reference. He overcame the apparently insuperable difficulty of the concave eye-glass, by grinding the edge of that lens into a convex form, in the focus of which the wires of the telescope were placed, and to which the magnified image of exterior objects could thus be referred.

Amici made original observations of value in the circulation of the sap in *Chara Vulgaris*, and is said to be the discoverer of the pollen tubes in plants of the phanerogamic group.

On the 6th of January died Ernesto Capocci, Director of the Observatory of Capodimonte. This excellent man had been for some time suffering from an internal complaint, which in the last few months increased and brought on disease of the heart, which terminated his painful sufferings. He was born on the 28th March, 1798, at Picinisco, in the province Terra di Lavoro. He went, early in life, to reside with his maternal uncle, Zuccari, Director of the Observatory of S. Gaudioso, from whom he received his scientific training, while he was indebted to the Abbé (afterwards Cardinal) Capaccini for the literary culture manifested more especially in his later works. In them the clearness of scientific enunciation is always coupled with the charm of a poetical diction which rendered his writings peculiarly attractive. Quick at seizing a new discovery, he knew how to give it an interesting turn, and to describe it in clear and unmistakable language, which gained for him many readers.

In youth, Capocci's scientific activity was already developed; for in his eighteenth year he had published, in the *Giornale Enciclopedico di Napoli*, some astronomical observations: he was also a voluminous contributor to *Zach's Correspondence Astronomique;* and he was a diligent computer of the orbits of comets, following up closely with calculations the tracks of these erratic bodies.

With a critical eye he watched the spots of our luminary, and his observations concerning them are lucid and important. His treatment of the Hora 18 of the Berlin Star Maps, which was one of the most difficult on account of the number of stars, obtained for him well-deserved commendation. His *Dialoghi*

sulle Comete, to which the five comets of 1828 gave rise, afford an example of classical style, and answer in a lucid manner the various questions which the fresh appearance of a comet gives rise to.

He became Director of the Observatory of Capodimonte in 1838. During the years 1836 to 1858 he travelled in France, England, and Belgium. He sat in Parliament under Ferdinand II., but had to resign his office of Director of the Observatory on the breaking up of the Constitution; and it was not until 1860 that he was again installed in it. During the period of his privation and trial he wrote, in a colloquial form, his commentary relating to the cosmography of Dante's *Divina Commedia;* and it would be difficult to point out any one better fitted for such a work than Capocci, who combined with high scientific knowledge rare literary talent.

On the consolidation of the present kingdom of Italy, Capocci was appointed Senator, and was created Commander of the Order of St. Maurice and Lazarus.

Honourable in the highest degree, Capocci was remarkable for the unassuming simplicity of his manners, his amiability and kindliness. Always modest, he was lively in conversation, and had the faculty of drawing towards him all who knew him. The goodness of his heart was always pourtrayed in his intelligent features, and even death did not efface their sweet expression.

Ottaviano Fabrizio Mosotti was born at Novara on the 18th of April, 1791. He studied with great ardour and success in the College of Monza, and in 1811 he gained the laurel prize for Mathematics in the University of Pavia, where Brunacci said of him, "For two years he was our scholar, but we cannot deny he has become our master." At Pavia he remained until 1813, hoping to obtain a chair in one of the Italian universities, but failing in this wish he determined to study engineering; yet, in order to preserve his scientific acquirements, he attached himself to the Observatory at Milan, retaining his connexion there until 1823. In the interim Mosotti published several memoirs on various scientific subjects, and was in 1822 elected one of the forty members of the Italian Scientific Society.

From the known liberality of his political sentiments he soon fell under the suspicions of the Austrian Government, and was ultimately compelled, like many others, to seek for safety within the shores of England. Here he naturally became acquainted with many of the chief scientific men of the day, and was enrolled on the list of Foreign Associates of the Royal Astronomical Society.

In 1827 Mosotti was appointed, while in London, to the Mathematical Chair in the University of Buenos Ayres; owing

to political disturbances he never occupied it, but after some time was elected Chief Engineer and Assessor in the Topographical Office of that city, and afterwards to the Chair of Natural Philosophy. Here Mosotti gained universal esteem, and the good opinion of even the Dictator Rosas himself, and in after times he often referred to this portion of his life with peculiar pleasure.

But Mosotti's affections were far away, in his native country, and ever longing to return; in 1835 he embraced the honourable opportunity which was presented to him by Cardinal Oppizzoni, the legate of Bologna, of presiding over the University of that city. On his return to Italy, the Roman Government, acting, it is presumed, under the dictation of Austria, refused to confirm Mosotti in the appointment which he had accepted, the Papal authorities, after repeated expostulations, presenting him with 2500 scudi as some compensation for his loss.

In 1836 Mosotti gave to the world his treatise on molecular action, for which he is so justly celebrated, entitled, *On the Forces which regulate the Internal Constitution of Bodies.*

In 1837 Mosotti was appointed to the Professorship of Mathematics in the University of Corfu, and while there he succeeded, as was natural, in securing the esteem of many of the English residents. A mutual attachment here sprang up between himself and a young English lady, Miss Anna Suter; and in 1840, on his election to the Chair of Mathematical Physics in the University of Pisa, she became his wife. He was a loving and exemplary husband, until the death, at Pisa, in 1843, of at once his wife and her infant.

Mosotti was always a welcome and remarkable member of those meetings of learned men which kept alive the national spirit of Italy, and paved the way for the movements of 1848. During his professorship at Pavia Mosotti published many valuable papers on scientific subjects, but these pursuits could not make such a heart as his forgetful of his country. He sat in the Tuscan Senate, and in 1848 he commanded with signal bravery the University Brigade in the war with Austria. At Curtatone and Montanara a powerful Austrian force attacked the Tuscan camp. In the thick of the combat Mosotti remained unmoved, but the brigade under his command was soon surrounded by the Austrian artillery, and placed in the greatest peril. When entreated by his pupils to save his life, he replied, "Do you think I, who am your father, will abandon my sons in their peril?" At the end of the war the brigade which loved him so well dispersed, and Mosotti returned to his studies once more.

Mosotti was a Senator in the Italian Parliament at Turin, but the fatigue of the journey to and fro was too much for him, and he rarely sat in his place. So in the softer air of Pisa he

continued to study and write, until in 1863 he was attacked with rheumatic fever, and died on the 20th of March in that year.

Pisa, which had watched over him as over a favourite child, mourned him with deep and universal regret. All had known and loved the professor; and almost every household mourned, when, the day after his death, his body was borne to the Campo Santo, followed by the members of the University, the brigade that he had commanded in 1848, the National Guard, officials, civil and military, and an immense crowd of private persons, who wished to show their veneration for the great man who was dead.

Senator Baron JEAN BATISTE PLANA was born in Voghera in November 1781.

He entered the École Polytechnique of Paris, classed eighth among 120 admitted under Professors Lagrange, La Place, and Legendre.

On leaving the Polytechnic School he was appointed professor in the Ecole Impérial d'Artillerie of Alexandria in Piedmont, and was sent there by Legendre with the following remark, "Vous êtes jeune, mais la jeunesse est un défaut dont on se corrige tous les jours."

From 1812 to 1853 Plana was Professor of the Higher Mathematics in the University of Turin, and was also Professor at the Military College. He was appointed Royal Astronomer to the Observatory of Turin, and held that office until his death. He was President of the Academy of Sciences in Turin, Membre Etranger de l'Institut de France, and member of most of the existing scientific societies.

Plana married Alexandra, daughter of the celebrated Lagrange. He had one son, who died in early life, and a daughter, Sophia, still living.

He died in Turin on January 20th, 1864, where he was buried with almost regal pomp.

Plana appears to have directed his attention for many years to the Theory of the Moon's movement; for, though his great work on that subject was not completed until 1832, yet so early as 1816 his friend the Baron de Zach wrote to him as follows:—"I am aware that you have undertaken, in concert with Signor Carlini, an important work on the Theory of the Moon. The existing theory has, in truth, much need of revision, and I have no doubt much good will result from your investigations."

Plana and Carlini were associated as astronomers in the measure of an arc of the Meridian in Savoy and Piedmont, entrusted to a mixed Commission, appointed by the Governments of Austria and of Piedmont, and performed during the years 1821, 1822, 1823.

This important operation was admirably conducted, and

the result published in two volumes; the Introduction to which was written by Plana, with the approval of Carlini. The astronomical labour and talent displayed in this work were so highly appreciated, that in 1828 the Institute of France adjudged the Lalande Prize to Plana and Carlini as authors of the most interesting observations and of the memoir most useful to the progress of astronomical science.

Plana was made a Baron by the King of Sardinia, and also named a Senator at the formation of that body in 1848.

Plana's first important work was his *Théorie Mathématique du Mouvement de la Lune*, and the latest, *Mémoire sur les Formules du Mouvement circulaire et du Mouvement elliptique libre autour d'un point excentrique par l'Action d'une Force centrale*, was read by the veteran *savant* himself at the Academy of Turin a short time before his death, namely, on the 6th of January. Among his numerous contributions to science may also be cited a mathematical demonstration of the existence of a circumpolar northern ocean. Plana was always accessible to strangers visiting Turin, and was remarkable for his kind and courteous reception of men of science who sought his acquaintance.

MAXIMILIAN VON WEISSE was born October, 1798, at Ladendorf, in Lower Austria, of which town his father was mayor. The family consisted of ten children (five sons and five daughters), Maximilian being the third.

In his tenth year he commenced his studies as a pupil in the Royal Academy at Vienna. He pursued the University course of studies, and took his degree as Doctor of Laws at the end of 1822.

Maximilian Weisse at an early age evinced a great inclination for mathematical and astronomical studies. On the 5th of August, 1823, he was named Assistant in the Royal Observatory of Vienna; and on the 24th of May, 1825, was appointed Professor of Astronomy and Director of the Observatory at Cracow.

In 1826, he married a daughter of a merchant of Nuremberg, residing in Cracow. He applied himself to his scientific pursuits in the University of Cracow with great diligence. Weisse was Dean of the Faculty of Philosophy in the Sessions from 1833 to 1860; and from 1833 to 1847 he was a Member of the Senate of the University.

In May, 1861, Weisse quitted Cracow on account of ill-health, caused by the earnestness with which he had pursued his scientific labours. He received a liberal pension from the Government on his retirement. His malady gradually increased so that he was seldom able to leave his couch, yet he nevertheless continued to occupy himself with astronomical reductions and other scientific matters.

He died at Wels, in Upper Austria, on the 10th October, 1863.

Dr. Weisse's astronomical works are numerous. He laboured at his Catalogue diligently and successfully, and gained an enduring reputation as one of the most distinguished astronomers of his time.

Proceedings of various Observatories.

Royal Observatory, Greenwich.

No important changes, instrumental or otherwise, have been introduced into the Royal Observatory, Greenwich, during the past year.

An agreement has however been entered into between the Astronomer Royal and M. Le Verrier, Director of the Imperial Observatory of Paris, for a distribution of the labour of the Meridional Observations of the Small Planets. The Moon is observed at Greenwich with the transit-circle and altazimuth, on every night when its observation is possible. When the Moon passed the meridian between three and six o'clock in the morning, it was found that the same observer could not conveniently make the evening observations of Small Planets, and stars, and also those of the Moon and Moon culminators. At this part of the lunation, therefore, three observers were employed on the same day, and this, with reliefs on alternate days, pressed heavily on the observing staff of the Observatory. To diminish this pressure M. Le Verrier has undertaken the Meridional Observations of the Small Planets, from Full Moon to New Moon; Greenwich being answerable for their observation from New Moon to Full Moon. Captain Gilliss, the Director of the National Observatory, Washington, has also promised to co-operate in the Meridional Observations of the Small Planets.

The three Observatories of Greenwich, Paris, and Washington, will be able to obtain a sufficient number of meridional observations of all the known asteroids, within the reach of their meridian telescopes, if sufficiently accurate ephemerides shall be forwarded to them. Several Asteroids, in opposition during the past year, were not observed at Greenwich, solely for the want of sufficiently accurate ephemerides, and unless some efforts are made in this direction several of the asteroids will be lost.

The volume of *Astronomical Observations for* 1862 is printed, with the exception of the Introduction.

The observations of stars made from 1854 to 1860 inclusive, have been formed into a Seven-Year Catalogue. This Catalogue contains the places of 2022 stars, for each of which

precessions, secular accelerations, and star-constants have been computed afresh. The work is printed, and it is hoped that it will shortly be ready for distribution. This Catalogue comprises about 400 stars contained in Bessel's *Fundamenta*, but not contained in the Greenwich Twelve or Six Year Catalogues, Bradley's places have been brought up by Mr. Stone to the epoch 1860 of the new Catalogue, and the resulting Proper Motions have been laid before this Society.

The application of the Astronomer Royal's method of determining the Motion of the Solar System in Space to 1167 stars, commenced several years ago at the Royal Observatory, has been brought to a conclusion. The result has been laid before the Society by Mr. Dunkin, and will be found printed in the *Memoirs*.

The Equatoreal observations of *Mars*, east and west of the meridian, made during the opposition of 1862 for the determination of the Solar Parallax, have been discussed; the number of corresponding observations is small, and most of these were made under unfavourable atmospheric circumstances; the result is of little weight, but, as far as it goes, it tends to confirm the results lately obtained from declination observations of *Mars*.

The declination observations of *Mars*, and stars made during the same opposition, have been compared with the corresponding observations at the Cape of Good Hope and Williamstown. The separate results are confirmatory of each other.

The great Equatoreal has been employed during the past year in observations of the fixed lines of the stellar spectra. The general results have been laid before the Society. Since the observations printed in the *Monthly Notices* were made, the prism-apparatus has been provisionally altered, so that, instead of producing astigmatic breadth of the spectrum, by the unequal refractions of a conical pencil at the two sides of the prism, a pencil of rays made parallel by a lens traverses the prism, and after being made convergent by a second lens is made astigmatic by a cylindrical lens. The definition of lines appears to be improved, and the facility of measuring them increased. The Equatoreal has also been used in observations of the Nebula of *Orion*, a drawing of which is to be made by Mr. Carpenter, and in a few observations of the solar disk with a Hodgson's eyepiece.

On the evening of January 11th the Nebula of *Orion* was splendidly seen. It was the opinion of the observers, Messrs. Stone and Carpenter, that the portions about the so-called jaws were not so accurately represented in the late drawing by Professor G. P. Bond as in that of Sir J. Herschel, printed in the *Results of the Cape Observations*, 1847. There is a squareness of outline about the so-called jaws, and an almost total absence of luminosity within them, in the drawing of Professor

Bond, that certainly was not presented by the Nebula on this evening. The drawing of Sir J. Herschel appeared to represent as accurately as perhaps any drawing could, the appearances presented about the jaws; the only marked deviation being that the wisp of luminous matter, 10^s following and 300 divisions north in Sir John Herschel's drawing, appeared to point more towards the trapezium than there represented. The other parts of the Nebula have not yet been examined with the same care as the portions about the jaws.

The galvanic connexions of the Observatory have been still further extended during the past year. Currents from the Observatory now fire time-guns at Newcastle and Shields; and the Westminster Clock sends daily indications to Greenwich, by which its error is always known at Greenwich; currents in the opposite direction being sent from Greenwich, which enable the attendant to adjust the Westminster Clock to accuracy.

In order to get rid of the disturbing effects of changes of temperature on the magnetic instruments, the Astronomer Royal has determined to excavate a room underneath the present magnetic observatory, sufficiently large to contain the whole of the self-registering magnets. The work has been commenced. The apparatus for the photographic registration of earth-currents is nearly finished, and will shortly be ready for use.

The proposal for carrying a Railway through Greenwich Park was renewed in the last session of Parliament. The scheme was however withdrawn.

A new construction of heads of micrometers has been introduced in the Transit circle of the Royal Observatory, the milled heads being made conical, so that the pressure of the observer's fingers will inevitably force the micrometer's screw to its bearing. It is believed that the adoption of this construction may tend to remove every suspicion of imperfect bearing of the screw.

Royal Observatory, Edinburgh.

Meridian observations of stars, time-signalling, and Government meteorological calculations, have been the main work of the Royal Observatory, Edinburgh, during the past year. Besides this, the twelfth volume of printed *Astronomical Observations* has appeared, and the Astronomer thankfully records the assistance afforded by the Royal Astronomical Society in distributing many of the copies. An addition, of an unusual character, made to the volume is worthy of notice, viz., four plates, photographic, and one, photoglyphic; the latter specially prepared for the occasion by Mr. Fox Talbot, the venerable inventor both of photography and photoglyphy. The scientific reason for the introduction of these plates, which are highly

magnified portions of some of the Teneriffe photographs of 1856, is the remarkable testimony which they bear to the transparency of the atmosphere and its suitability to telescopic observation at great heights above the sea-level.

In the department of time-signalling, some important extensions have been made during the past year, almost involuntarily on the part of the Observatory, but strictly in accordance with the original instructions to the Astronomer Royal for Scotland. Of the three methods which are practised in the Observatory,—viz., by electric-controlled clocks (R. L. Jones's patent), and of which there are now no less than six; electric-dropped time-ball; and electric-fired time-gun,—the first and the third seem to be most in public demand. The Edinburgh time-gun, at a distance of 4000 feet from the Observatory, is fired by a special application of electricity, which, though apparently the very best that could be contrived for that case, does not suit very much greater distances, as it implies the continued and permanent use of a wire connecting the stations. Hence it occurred to Mr. N. J. Holmes, Telegraphic Engineer, in the course of the last spring, to elaborate a method, based on the use of Professor Wheatstone's magneto-exploder and Mr. Abel's magnetic fuse; and having obtained promise of assistance from the Royal Observatory, Edinburgh, the experiments went rapidly forward. The first point was, to ascertain the trustable amount of exploding power available in this manner along the ordinary open lines of telegraphic wire. Accordingly, with the further help and countenance of the Electric and International Telegraphic Company, a previously arranged series of currents was sent off from the magneto-exploder (in external appearance merely a box of about 8 inches cube) in Edinburgh, to the Company's Office in Newcastle, a distance of 120 miles; and the result in the times and intervals, as tested by a chronometer belonging to Messrs. Reid, of that city, showed no sensible hesitation or hang fire.

This notable experiment settled the great power of Wheatstone's exploder in overcoming the resistance of a very long wire when its insulation was good; but when that was bad, as happened only the following day, by reason of a heavy fog, the high intensity of the magnetic currents caused their loss and dissipation before reaching Newcastle. Hence a practical system was soon devised, by which a large signal-sending clock, recently prepared for the Royal Observatory, Edinburgh, by Messrs. Ritchie and Sons, clockmakers there, despatched along the line a current of galvanic electricity of low intensity; and this, on reaching Newcastle, automatically liberated there a current of *magnetic* electricity, which had then only a few hundred yards to travel, through a covered wire, to the place where the time-gun was placed.

Professor Smyth, with the aid of local subscriptions, ex-

tended his experiments to other cities in England and Scotland; altogether, no less than seven different "distant" time-gun signals have been successively—though they are not all at the present moment—fired, directly from the Royal Observatory, Edinburgh, and day by day further inquiries are being made from other quarters, and five more cities are already in pretty active preparation; while Government, with a truly liberal discernment, has looked on the system so favourably as to lend guns, 24-pounders, to nearly every city that has fully subscribed amongst its citizens for the expense of powder and maintenance.

The Royal Observatory, Edinburgh, has been consequently obliged to enlarge its galvanic batteries and telegraphic connexions, and has drawn up a set of rules and a system of supervision, including a weekly schedule report, for any distant time-gun, where Edinburgh currents are expected. By strictly insisting on these rules, those cities in connexion with the Edinburgh Observatory will have the satisfaction of knowing, that, however distant they may be from Edinburgh, their time-signals are fully as prompt and exact, even to a very small fraction of a second, as if they had a Royal Observatory within a few feet of them.

Radcliffe Observatory.

No change whatever has occurred in the operations of the Radcliffe Observatory since the publication of the last Annual Report. The staff of Assistants remains the same, and the subjects of observation are essentially the same as they were in the preceding year.

The Carrington Transit-circle has been used without intermission in the continuation of the observation of the stars of this hemisphere lying between the fifth and seventh magnitudes, and in observations of the Sun, the Moon till past opposition, and some of the planets. Mr. Main is still well satisfied with its performance; and, as stars north and south of the zenith are ordinarily observed by reflexion and direct vision, in the same manner as at Greenwich, and the errors of collimation and level are observed every evening with indisputable accuracy in the same way, there is every guarantee for the goodness of the results, so far as this depends on the instrument. The graduations of the circle were examined in January 1863, by means of several series of readings with the eight microscopes, at intervals of 5°, and the result is very satisfactory, as showing that the errors are very small.

A great many occultations of stars have been observed, and the times of beginning and end of the solar eclipse of May 17, 1863. All these instances, amounting to nearly twenty in

number, have been rigorously discussed by Mr. Main, by a peculiar method which he believes to offer great facilities for the calculation of this class of phenomena, and the results all show that the errors of the Moon's places throughout the year, as given by Hansen's Lunar Tables, are very small.

With the heliometer, observations of double stars have been rigorously continued, and the number of stars observed (additional to those of preceding years) is about 190. The stars chosen for observation in 1863 were chiefly the *Lucidæ* of Struve, of the whole of which the re-observation at the present time seems desirable. The components of these systems are, it may be remembered, of nearly equal magnitudes, varying from about the sixth to the ninth; and Struve concluded that there was very great probability that the larger number, if not the whole of them, were physically and not optically connected. The results of the Oxford observations thus far do not confirm this idea, as, in the interval of more than thirty years which have elapsed since Struve's observations, very few of them exhibit any considerable motions in distance or position-angle. Mr. Main proposes to complete the re-observation of the whole of them, and the discussion of the results cannot fail to be interesting at the conclusion of his labours. Almost all the double stars in Smyth's *Cycle*, which it seemed desirable to re-observe, have had proper attention paid to them.

In the autumn of 1863, new machinery for opening and closing the shutters of the dome of the heliometer was supplied by Mr. Grimsley, of Oxford, and the shutters will now fall back far enough to allow of the instrument being used equally well in all positions. The time occupied in making this alteration was nearly three months; and, of course, during this interval the instrument was out of use.

The photographic meteorology has been kept up with the same vigour as in former years, under the charge of Mr. Lucas. During the heavy gales which occurred in October and December, an interchange of copies of the photographs giving the meteorological elements was made between Greenwich, Oxford, and Kew; and the comparison of the critical points of the storms was found to be very instructive and interesting.

The twenty-first volume of the *Radcliffe Observations*, containing the observations of the year 1861, has recently been published; and the Council are glad to draw attention to this volume, not only for the value of its contents, but also because it tends to show that the practice, for so many years adhered to by Mr. Johnson, of reducing the observations and publishing the results as soon as possible after the close of any year, is still kept up by the present Director. When it is considered that, in the space of very little more than three

years, the results of the observations of four years, together with the Radcliffe Catalogue of Stars, have been given to the world, and that the reductions for 1862 are in a very forward state, there appears to be little doubt that the existing establishment will still be able to make the reduction keep pace with the observations, and that early publication will still continue to be one of the characteristics of the Radcliffe Observatory.

The reductions for 1862, as before stated, are in a very forward state, but those of the meridian observations for 1863 have not been commenced yet. Indeed, the Director is conscious that his own energies, as well as those of the Assistants, have been taxed to the utmost in the production of the amount of work which has been reported within the last two years, and that probably a diminution of some of the heaviest portions may become necessary, keeping in mind the necessity of making the reductions keep pace with the observations, and of publishing a volume annually. He is also not unmindful of the importance of commencing, without further delay, the compilation of the second *Radcliffe Catalogue of Stars*, which will include all the stars observed with the transit-instrument and the meridian-circle, from the year 1854 to 1861 (both inclusive).

At all events, the works which are now in progress will prevent any anxiety, for some years to come, about the activity and usefulness of this Observatory.

Cambridge Observatory.

The meridian observations made at this Observatory during the past year comprise observations of the Sun near the equinoxes and solstices; observations of the planets *Jupiter*, *Uranus*, and *Neptune;* observations of stars which were compared with comets in the years 1861, 1862, and 1863, and also of stars selected from Oeltzen's comparison of Lalande's *Histoire Céleste* with Argelander's *Northern Zones*, for the purpose of testing all the more considerable proper motions thus found by M. Oeltzen. With the equatoreal have been made observations of Comets II., III., and IV., 1863, and a few observations of occultations of fixed stars by the Moon, and of eclipses and other phenomena of *Jupiter's* satellites.

The work of the Observatory during the past year has been seriously crippled by the loss of the First Assistant. Mr. Bowden left in January, 1863, in order to enter upon an occupation wholly unconnected with astronomy. Some difficulty has been found in supplying his place, as Prof. Adams was anxious to secure the services of an Assistant who had already had experience in observatory work, and it was hardly to be expected that this could be done without a considerable

augmentation of the stipend usually paid. This difficulty, however, has just been removed by a grant from the Sheepshanks Fund; and Prof. Adams has been fortunate enough to secure the assistance of Mr. A. Graham, whose labours at the late Mr. Cooper's Observatory, at Markree, have made his name so well known to the astronomical world. Under these circumstances it may be confidently anticipated that this Observatory is about to enter upon a period of renewed and increased activity.

Some alterations have been made at the eye-end of the Northumberland Telescope, in order to render it possible to use eye-pieces giving a larger field of view; and a small position-circle has been applied to the finder, so as to give the means of measuring approximately angles of position, such as that of the tail of a comet which could not be observed in the large instrument.

The Jones Equatoreal has been put into working order by Mr. Simms; and the Graham Clock, which is used with this instrument, has been thoroughly restored by Mr. Frodsham.

The printing of Vol. XX., containing the Meridian Observations to the end of 1860, is now complete, with the exception of the Introduction, which Professor Challis is preparing for the press.

Liverpool Observatory.

The site on which the Liverpool Observatory now stands will shortly be required for dock accommodation, and it will be necessary either to remove the building some two or three hundred feet to the south of its present position, or to find a more eligible site.

Equatoreal observations have been taken of Comets II., V., and VI., 1863, but they have not yet been reduced. The new self-registering barometer has been in operation since June last: the sheets on which the record is obtained are removed from the cylinder every morning at 9 A.M., and a tracing from the original for the previous twenty-four hours is forwarded daily, at 10 A.M., to the Underwriters' Rooms, together with an account of the force and direction of the wind, the fall of rain, &c. A meteorological record is also forwarded regularly to the Mersey Docks and Harbour Board.

It was stated last year that considerable alterations had been made in the method of giving the errors and rates of chronometers; and, although the docks in the immediate neighbourhood of the Observatory have for some months past been closed to sea-going vessels, the increase in the number of ships' chronometers sent to be rated has been considerable. This Mr. Hartnup attributes to the new method of giving the errors

and rates being more highly appreciated. During the winter months each chronometer is exposed for a week to the temperatures 50°, 65°, and 80° alternately; and for whatever time the chronometer may be at the Observatory the error is given at the end of each seven days, together with the mean rate and extreme difference of rate between any two days for each week. The latter, Mr. Hartnup thinks, shows the quality of a chronometer better than any other method that he has been able to devise. The system of clock-controlling, and the method of communicating time to the port generally, as mentioned in former notices, continues to give great satisfaction.

Glasgow Observatory.

Since the installation of the Ochtertyre Equatoreal at the Glasgow Observatory, the attention of Professor Grant has been chiefly occupied with the negotiation of arrangements for the transmission of true time from the Observatory to the City and Port of Glasgow, and the shipping of the Clyde in general. An electric wire has recently been laid down, connecting the Observatory with the University buildings; and two clocks—one the turret-clock of the College, the other a small clock in Professor Thomson's Class-room—have been placed under the control of the mean-time clock of the Observatory by the application of Jones's well-known method. The result, in both cases, has been completely successful. The Town Council have commenced operations with one of the City clocks, for the purpose of similarly placing it under the control of the Observatory clock; and there is every probability, at present, of Jones's invention acquiring, ere long, an extensive application in Glasgow and the neighbouring towns, in connexion with the Observatory of the Glasgow University. It is intended by Professor Grant that the fine Equatoreal, which has been mounted at the Observatory, shall be devoted chiefly to measures of Double Stars and differential observations of the Minor Planets. A micrometer has been recently obtained from Mr. Simms, with a view to the prosecution of the last-mentioned object; and a course of observations has been commenced with it.

Mr. De La Rue's Observatory.

During the first three months of last year several very beautiful and interesting lunar photographs were taken; but in April the 13-inch silvered glass mirror, together with a silvered glass plane mirror, were received from Dr. Steinheil, of Munich, and the photographic records were discontinued in order to test the new mirrors.

The experiments, however, in enlarging the negatives to

the dimensions of Beer and Mädler's map (38 inches), which were commenced in 1862, have been continued in 1863, and results obtained far surpassing those previously recorded.

Hitherto the silvered glass mirror has not been used for astronomical photography, but it is contemplated to test its powers for that purpose as early as possible. The light reflected by the silvered glass mirrors is found to be considerably greater than that of the speculum-metal mirrors, and there is reason to believe that the time of exposure of the sensitive plate will be shortened. Up to the present time the mirrors have retained their original polish, the precaution having been taken to insert lime-boxes in the tube of the telescope, to prevent the mirrors from becoming damp.

As far as Mr. De La Rue has been able to judge, he does not believe Dr. Steinheil's mirror to be so perfect in figure as those surfaces he has been able to procure by means of the polishing machine described in the *Monthly Notices*.*

In February, 1863, Mr. Reynolds' engagement terminated, and he left Cranford; the Kew heliograph was then removed to the Kew Observatory, and re-erected in the dome of that establishment. Mr. Balfour Stewart, the Director, reports that it has been in regular operation at Kew since the middle of May last, Mr. De La Rue having undertaken to superintend the working of this instrument, with the aid of Mr. Bickley, of Kew, and a qualified photographic assistant. Up to the end of last year, 104 negatives, capable of being printed from, had been obtained, and of all these the definition is unexceptionable. The following list may be of interest to those who are engaged in procuring solar autographs: it comprises the dates of the pictures taken at Kew since the instrument was last mounted:—

Dates of Sun-Pictures taken at the Kew Observatory in 1863.

May 24th, 25th, 26th, 27th, 28th, 29th.

June 1st, 2d, 3d, 4th, 6th, 8th, 9th, 10th, 11th, 13th, 19th, 20th, 23d, 25th, 26th, 27th, 29th, 30th.

July 1st, 2d, 3d, 4th, 5th, 6th, 7th, 9th, 10th, 12th, 14th, 15th, 17th, 18th, 19th, 24th, 26th, 28th, 29th, 30th, 31st.

August 1st, 5th, 7th, 8th, 10th, 11th, 12th, 22d, 24th, 28th.

Sept. 1st, 7th, 10th, 11th, 16th, 17th, 18th, 19th, 22d, 23d, 24th, 25th, 26th, 28th, 29th, 30th.

October 1st, 5th, 6th, 13th, 14th, 16th, 17th, 22d, 23d, 31st.

November 2d, 6th, 9th, 12th, 13th, 19th, 20th, 21st, 22d, 23d, 28th.

December 3d, 4th, 10th, 12th, 14th, 16th, 18th, 19th, 22d, 23d, 25th, 30th.

In all 104 Pictures.

* Vol. xiii. p. 44.

The photographs of the Sun's disk, obtained at Cranford from Feb. 7, 1862, to Feb. 7, 1863, amounted to 213. With the kind permission of Mr. Carrington, Mr. De La Rue engaged the services of Dr. von Bose to measure these photographs, and to reduce the measurements to heliocentric positions of latitude and longitude. The measurements were made by means of Mr. De La Rue's micrometer, described in the *Philosophical Transactions*, vol. 152, pp 373–375.

With the exception of six of the photographs, which were too faint to be adjusted, all the disks have been measured, so that measurements of 207 pictures, obtained in 161 days, have been completed. Whenever there was only one photograph for a day, this has been adjusted and measured twice through, and 1880 positions of Sun-spots have been obtained.

The measurements and sketches of the spots have been entered into ruled sheets, and again checked with the originals.

The reductions of the 1880 positions (according to Mr. Carrington's methods) are finished, but there is a good deal of checking to go through. The resulting positions have been put together on separate sheets, but will also require to be gone through before they are ready for publication.

The series of photographs form a continuation of the observations recently placed before the scientific world, by the publication of Mr. Carrington's valuable work, and it is to be hoped that the photographic observations and reductions will not be allowed to drop now that it has been demonstrated that they afford accurate numerical results.

Ely Observatory.

The "Heliautograph" of the Rev. W. Selwyn, consisting of a photographic camera, attached to a Dollond refractor of $2\frac{3}{4}$-inches aperture, has been in operation at Ely from the 1st Jan. 1863; the Royal Society having made a grant of 50*l.* to enable Mr. Titterton to devote himself to the work of obtaining a regular series of solar autographs.

The number of days from Jan. 1, 1863, to Dec. 31, 1863, is 139, on 55 of which two negatives were taken—the second after a short interval of time—with the view of ascertaining the true character of any doubtful spot.

In this series there is no such long and close succession of disks as was obtained between July 25 and Aug. 4, 1862, when a very large spot, with its train, was exhibited on eight days of its passage from the eastern to the western limb, as seen in the frame which hangs in the Society's room; but there are two interesting records of the solar eclipse of May 17, 1863, one near the beginning, $5^h\,46^m\,35^s$ G.M.T., the second near the middle, $6^h\,20^m\,10^s$ G.M T.

The latter, being taken in a clear sky, shows very distinctly the uneven edge of the Moon on the solar disk, and the appearance (so often noticed, and now, it is believed, generally recognised as an optical effect) of a bright band surrounding the edge of the Moon.

The images of the solar disk are about 4 inches in diameter, and the whole series, placed in frames to allow of easy comparison by the eye, will be exhibited to the Society before they are presented to the Royal Society in acknowledgment of their grant.

It has not yet been found practicable to add to these autographs the lines of position for Right Ascension and Declination, as in the Kew instrument; but it is hoped that, while the Kew pictures serve the purpose of ascertaining the exact positions and measuring the proper motions of the spots, the Ely autographs will have some value, as recording the physical phenomena of the formation, the changes, and the dying away of spots; and it may be observed, in particular, that the *faculæ*, so intimately connected with the spots, are here well seen.

The Kew and Ely series will supplement each other on several days of 1863, and copies for those days are to be transmitted from the one to the other. The whole number of Solar disks recorded in the two series is 188.

During the past year, Mr. Titterton has also given much of his time to the getting into working order a larger refractor, of 6 inches aperture, with camera attached. After many trials and difficulties, he has succeeded in obtaining the chemical focus, and hopes soon to show solar autographs of 6 inches diameter. It is intended to use this instrument for the delineation of spots and groups, when near either limb, bringing that portion of the disk as near as possible to the centre of the photographic plate. This instrument is so made that autographs can be taken either with or without the lines of position.

The Progress of Astronomy during the past Year.

The past year, if not remarkable for salient astronomica discovery, can scarcely fail to be memorable for the activity displayed in the many contributions which during its cours have been made towards the perfection of astronomical ac curacy. The most important elements of our cosmical systen are better known to us at its close than they were at it beginning; we have profited largely by the stores of know ledge treasured up for us by the labours and genius of astro nomers, whose day of toil has long since closed; and we ar every year becoming more sensible of the intimate relatio

in which every branch of natural science stands to our own favourite pursuit, the chief of them all.

The correction made to the Sun's Parallax may justly claim the foremost place in the annals of our astronomical year. The received value of this element, 8″·56, adopted by Encke from questionable observations of the last transit of *Venus* in 1769, has always been accepted by astronomers as simply provisional; and, in the apparent absence of more available means, they have been patiently waiting until improved methods and better instruments could be applied to the next occurrence of the same phenomena in 1874 and 1882. Meanwhile this very correction, thus patiently waited for, had been shown by Le Verrier to be all along, by implication, contained in the comparison between the theoretical and observed motions of *Mars*, *Venus*, and the Moon; and the improved value thus suggested by theoretical considerations was adopted in his Solar Tables. Before him, Hansen had in the year 1854 announced that the parallactic inequality in the lunar theory indicated the necessity of increasing the solar parallax. Subsequently to Hansen and Le Verrier, M. Leon Foucault has succeeded in determining the velocity of light by certain mechanical arrangements of considerable simplicity and easy repetition, and combining his result with the well-ascertained coefficient of aberration he has deduced a value of the solar parallax in satisfactory accordance with the amount assumed by Le Verrier. Mr. Stone, on comparing the observations of *Mars* at his recent opposition, made at Greenwich with similar observations made at Williamstown, Australia, has deduced a result for the solar parallax possessing a remarkable coincidence with those obtained by the other astronomers from theoretical considerations. Le Verrier's value is 8″·95; Hansen's 8″·9159; Stone's, as deduced from *Mars* at opposition, 8″·932 ± 0″·032. (*Monthly Notices*, April and June, 1863.)

M. Winnecke, from a comparison of the Pulkova and Cape of Good Hope Observations of *Mars*, obtained the value 8″·964 (*Ast. Nach.* 1409).

This minute correction, amounting to no more than two-fifths of a second of arc, thus curiously brought to light in the first instance by small disturbances in the motion of the Moon and planets, may reasonably inspire astronomers with additional confidence (if that were needed) in the exactness of their science and in the fixedness of the laws which bind the Kosmos together. And if, on the other hand, a contrary misgiving is created in other minds from the fact that this abrupt alteration of so important an element as the solar parallax implies an alteration of some four millions of miles in the Sun's reputed distance from our Earth, this misgiving may perhaps be removed by the consideration that, after all, this improvement of our knowledge amounts to no more than a correction to an observed angle

represented by the apparent breadth of a human hair viewed at the distance of about 125 feet.

Coincident in time with our improved knowledge in the distance of the Sun, comes curiously enough that of the Moon also. Mr. Breen, in the volume of our *Memoirs* for the present year, has compared observations of the Moon's zenith distance, made at the Cape of Good Hope during the years 1834, 1836, and 1837, with similar contemporaneous observations made in Europe. The result is, that Mr. Adams's constant of Lunar Parallax, viz. 3422″·32, requires a correction of + 0″·38, and therefore becomes 3422″·70. This latter constant is adopted in the *Nautical Almanack* for 1867; and, it may be observed in passing, implies a diminution of our mean distance from the Moon of about 26 miles. It is no more than what is due to Professor Adams, to quote from his Memoir in our *Monthly Notices* for April 1853, the following clause, as if in anticipation of this improvement upon his own labours; "Mr. Maclear's observations," he says, "at the Cape, combined with European observations, would doubtless furnish most valuable materials for a new determination of the constant of parallax." To this remark it may be added, that a still closer approximation to the truth is to be expected from observations made with the noble transit circle, which has happily displaced Jones's mural circle at the Cape.

Astronomers will regard with especial interest the Astronomer Royal's renewed attempt to determine the magnitude and direction of the motion of the Solar System in Space. Sir W. Herschel in 1783, by a graphical method of great simplicity, showed that the proper motions of a few stars might be tolerably well accounted for, by assigning to the Sun a motion of his own directed towards λ *Herculis*. Other astronomers, starting with this as an approximate apex of solar motion, have sought to correct it by combining together a far greater number of stars than could be taken into account by the elder Herschel. The Astronomer Royal, by the independent method of referring all the motions to three rectangular co-ordinates, as applied to 1167 stars, falls again very nearly upon Sir W. Herschel's original position of the solar apex. And yet, strange to say, notwithstanding the near coincidence of all the results of the before-mentioned independent methods of investigation, the inevitable logical inference deduced by Mr. Airy is, that the whole question of solar motion in space, so far at least as accounting for the Proper Motion of the stars is concerned, appears to remain at this moment in doubt and abeyance. A comparison of Sir W. Herschel's exquisitely simple memoir in the *Philosophical Transactions* for 1783, with the Astronomer Royal's elaborate investigation, as exhibited by Mr. Dunkin in the present volume of our *Memoirs*, will be found replete with interest and instruction in this grand cosmical problem.

In no remote connexion with the foregoing question we may record Goldschmidt's discovery of one or more companions to *Sirius*. Last year we had occasion to remark that Bessel's hypothesis of non-luminous or hitherto invisible companions to *Sirius* and *Procyon*, had been shown by Auwers and Safford satisfactorily to explain certain peculiarities in the Proper Motion of these stars, both in Right Ascension and in Declination. During the present year it is very remarkable that M. Goldschmidt should not only have confirmed Alvan Clark's discovery of a companion to *Sirius*, but observed also several others, and of these at least one has been verified by Mr. Dawes. (*Monthly Notices*, March and April 1863.) It is possible that one or more of these optically minute bodies may satisfy the conditions required. Whether this be so or not, the comparative insignificance of the instrumental means at the disposal of M. Goldschmidt, at once reflects honour upon that astronomer's zeal and sagacity, and affords peculiar encouragement to future observers unprovided with the costly luxuries of modern instruments. M. Goldschmidt's labours remind us of the slender appliances which in Dalton's hands were made to evolve some of the most memorable of chemical discoveries.

Nor have we yet done with the labours of the past year bearing at least some distant relation to the great cosmical question of the solar motion. Among the several tantalizing lacunæ in our knowledge which beset the investigation of this interesting subject, are especially two, viz., our ignorance of the Parallax of the stars, and especially our want of accurate knowledge of the Proper Motions of those in the Southern Hemisphere. There is now a hope that we see at least the commencement of an attempt to fill up both these lacunæ. On the one hand, M. Krüger (*Monthly Notices*, March 1863) has added two or three to the very limited list of stars whose parallax is approximately known. (*Monthly Notices* for March 1863.) On the other hand, we have reason to hope that the Council of our Society may be successful in rendering what assistance they can to the astronomers at the Cape, at Williamstown, and Madras, in the formation of a Catalogue of Stars in the Southern Hemisphere, similar to the one which Argelander has so ably completed for the north.

The cause of the well-known discordance between the zenith distances of celestial objects obtained by direct observation and those obtained by reflexion from quicksilver, was some time since suggested by M. Faye. The question, however, has this year been set to rest by the Astronomer Royal, who has successfully traced the cause to the meteorological condition of the air surrounding the instrument, as influenced by the dimensions of the shutters in the observing-room. The general conclusion drawn from the investigation has an important bearing on the accuracy of all instrumental determina-

tions of zenith distances, inasmuch as Mr. Airy has shown that all such determinations deduced from direct observation uncombined with reflexional observations are liable to an appreciable error, varying from a quarter to half a second. (*Monthly Notices*, June 1863.)

Correlative, and yet in a measure antithetical, to the foregoing effect of meteorological elements on the required accuracy of results, is the influence of surrounding *geological* conditions on the direction of gravity. The effect of mountainous tracts on the deviation of the plumb-line from the vertical has long since been ascertained; but it has been reserved for Professor Schweizer, of Moscow, to detect an error amounting to nearly 8″ in the direction for gravity, evidently caused by the lithological characteristics of a district not only totally free from mountains, but even presenting a very level contour.

It is, to say the least of it, a curious circumstance that astronomical results should be available for the detection of small relative *deficiencies* of matter, unsuspected by the eye, and extending over but a very few miles of the Earth's surface. (*Monthly Notices*, April 1863.)

No advance has this year been made towards the reconciliation of Professor Adams's value of the Acceleration of the Moon's mean motion with Hansen's value, which represents ancient eclipses so well; but Hansen himself has suggested that the hypothesis of the infinitesimal alteration of the time of the Earth's rotation by ·01197 seconds in 2000 years may possibly account for the discrepancy, and may also explain certain anomalies between the theoretical and observed places of *Mercury* brought to light by M. Le Verrier. (*Monthly Notices*, May 1863.)

The publication of the results of Mr. Carrington's scrupulously accurate and persevering labours on the Solar Spots will at all times be memorable in the annals of the physical history of the Sun. The methods adopted for reducing and tabulating the observations of the spots in their various phases deserve to be a model for future efforts. The influence which the varying distance of *Jupiter* may have upon these outbursts of solar activity, and the reactions which these in their turn may have upon the meteorological conditions of our own atmosphere; the apparent alternate expansion and contraction of the limiting parallels between which the spots are formed, and the curious drift in the strata of the photosphere which contain them, are among the interesting questions more or less briefly referred to in the volume before us, and the determination of which is necessary to the elucidation of Mr. Carrington's question, "What is a Sun?" It is the high reward of conscientious and well-directed researches such as these, that their results, far from being exhausted in the day of their completion, contain the germs of future and successive discoveries beyond the con-

templation of the original labourer. ("Observations on the Spots of the Sun," R. C. Carrington, 1863.)

In connexion with these phenomena in the solar envelopes we may refer to the discussions which have taken place at the meetings of our Society, and to two papers by Mr. Dawes in our *Monthly Notices* for December and January, relative to the telescopic appearance of the solar photosphere. A question has arisen whether the general appearance of the photosphere is that of a flocculent precipitate, as suggested by Sir John Herschel, and assented to by Mr. Dawes, or whether it more nearly resembles a willow-leaved crystalline precipitate of detached particles,* as originally described by Mr. Nasmyth, and confirmed by Mr. De La Rue and the writer of these pages. The question is remarkable, and worthy of the attention of observers possessing adequate optical means, not only on account of its important physical bearings, but as showing the extreme difficulty of a determination on which observers so eminent are found to differ.

Mr. Howlett has perseveringly continued his systematic observations of the physical features of Sun-spots, and, although possessing comparatively small instrumental power, has vied successfully with other observers in producing a series of Sun-records of great scientific value.

It would be disloyal to our Society were we to conclude this *resumé* of the astronomical labours of the past year without an allusion to the admirable work on Astronomy, recently published by one of the former Presidents. It is nothing more than an act of simple justice to the merits of Mr. Main's volume to say that it is the first successful attempt in this country to furnish the student, not only with the mathematical deduction of astronomical formulæ, but, at the same time, with the best methods of their practical application in the actual business of an Observatory.

* Particles! from 2″ to 3″ or 4″ long, by about half that breadth; the smallest of them having an area exceeding that of the British Isles. While these pages are passing through the printers' hands we hear of the confirmation of the existence of these strange entities, under convincing circumstances, by Mr. Stone, with the large Greenwich Refractor. To him the Sun's photosphere appeared as if covered with grains of rice. Perhaps, then, these *particles* are not quite so sharp as "willow-leaves" (*Nasmyth*), nor so round as "rice" (*Stone*), nor so long and jagged as "straw" (*Dawes*). Solid bodies, somewhat uniform in size and shape, the immediate origin of the solar light! Comparing great things with small, they remind the chemist of incandescent arsenic emitting light in the combustion of arseniuretted hydrogen.— [C. P.]

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Notes on 95 *Herculis*. Capt. Huggins.
Note on his Drawings of *Mars*. Mr. Lassell.
Observations of the Sun at Rising and of Solar Spots. Sir A. Lang.
Observations of Comet II. 1862. Sir T. Maclear.
Observations of η *Argus*. Mr. Abbott.
On certain Depressions on the Moon's Western Limb. Rev. H. C. Key.
Results of Meridional Observations of Small Planets, The Astronomer Royal.
Observations of *Venus* near her Inferior Conjunction. Capt. Noble.
On a Stand for a Reflecting Telescope. Rev. H. C. Key.
Errata in his Logarithms. Gen. Shortrede.
Elements and Ephemeris of Comet IV. 1863. M. Romberg.

Dec. 11. Elements of *Leto*. M. Luther.
Remarks on the Telescopic Appearance of the Exterior Envelope of the Sun, &c. Rev. W. R. Dawes.
On the Eclipses recorded in the Ancient Chinese Historical Work, called Chun Tsew. Mr. Williams.
On the Motion of the Solar System in Space. Mr. Stone.

June 12. On the Amount of Light given by the Moon at the greater stage in the excentrically Total Eclipse, 1863, June 1. The Astronomer Royal.
Observations of Minor Planets. The Astronomer Royal.

1864. Ephemeris of Comet V., &c., 1863. M. Romberg.

Jan. 8. Further Remarks on the Telescopic Appearance of the Exterior Envelope of the Sun. Rev. W. R. Dawes.
On the Calculation of the Optical Effect of Atmospheric Refraction. Prof. Challis.
Note on the Variable Nebula in *Taurus*. Mr. Hind.
Proper Motions of 459 Stars, determined from a Comparison between Bradley and the Greenwich Seven-Year Catalogue. Mr. Stone.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
The Secretary of State for War.
The Postmaster General.
Royal Society of London.
Royal Asiatic Society.
Royal Geographical Society.
Royal Institution.
Royal United Service Institution.
Geological Society.
Linnean Society.
Zoological Society.
Photographic Society.
Society of Arts.
British Meteorological Society.
British Association.
Institute of Actuaries.
British Horological Institute.
Art-Union of London.
Lancashire Historic Society.
Liverpool Mercantile Marine Association.
Literary and Philosophical Society at Manchester.
Royal Society of Edinburgh.
Royal Dublin Society.
Royal Observatory, Edinburgh.
Royal Society, Victoria.
Imperial Observatory, Paris.
Imperial Observatory, Vienna.
Imperial Observatory, Königsberg.
Royal Observatory, Munich.
Royal Observatory, Brussels.
Royal Observatory, Madrid.
Observatory, San Fernando.
Observatory, Bonn.
Observatory, Christiania.
Observatory, Mannheim.
Observatory, George Town College.
L'Académie Impériale des Sciences de l'Institut de France.
Le Dépôt Général de la Marine.
L'Académie Impériale de Dijon.
Royal Academy of Sciences, Berlin.
The Society of Physics, Berlin.

Royal Academy of Sciences, Munich.
Royal Academy of Sciences, Brussels.
Royal Academy of Sciences, Göttingen.
Royal Academy of Sciences, Amsterdam.
Royal Academy of Sciences, Lisbon.
Royal Society, Naples.
Royal Danish Academy.
Imperial Academy, St. Petersburg.
The Society at Altenburgh.
The Royal Saxon Society, Leipsig.
Austrian Board of Marine.
Canadian Institute.
Committee of Harvard College.
Observatory of Harvard College.
American Academy of Arts and Sciences.
American Philosophical Society.
Academy of Natural Sciences, Philadelphia.
United States Naval Observatory.
Smithsonian Institution.
Franklin Institute.
Committee of the American Coast Survey.
Editors of Silliman's Journal.
Editor of the Athenæum.
Editor of the Parthenon.
Editor of the Critic.
Editor of the London Review.
Editor of the Reader.
Editor of the Intellectual Observer.
Editor of the Sculptor's Journal.
Editor of Cosmos.

G. B. Airy, Esq.
M. Auwers.
J. Baxendell, Esq
Dr. Beke.
Messrs. Bell and Daldy.
M. P. Béron.
G. P. Bond, Esq.
W. C. Burder, Esq.
T. W. Burr, Esq.
Dr. Socrate Cadet.
M. Delaunay.
Prof. De Morgan.
S. M. Drach, Esq.
Prof. Encke.
Dr. Francis.
M. le Prof. Gautier.
S. Gorton, Esq.
Lt.-Col. Graham.
Prof. Hansteen.
J. Hartnup, Esq.
Prof. Heis.
J. Herapath, Esq.
Sir J. F. W. Herschel.
G. W. Hough, Esq.
M. Houzeau.
G. Knott, Esq.
Dr. E. Luther.
W. T. Lynn, Esq.
M. E. Mailly.
G. Maughan, Esq.
Rev. R. Main.
M. Mettenius.
M. L'Abbé Moigno.
Dr. Oudemans.
Dr. Peters.
M. Petit.

M. Plana.
Prof. Plantamour.
Prof. Quetelet.
Sig. Santini.
M. Scarpellini.
M. Schiaparelli.
M. Schwabe.
M. Schweizer.
Prof. Secchi.
Messrs. Smith and Beck.
Prof. W. Von Struve.
M. O. Von Struve.
C. Todd, Esq.
M. Villarceau.
Mr. J. Williams.
M. Winnecke.
Dr. Witt.
Dr. R. Wolf.

The Chairman, after the conclusion of the reading of the Report of the Council, rose to offer a few observations to the meeting. He said that he had an announcement to make to them which would probably produce a chill of disappointment in many of the members. The annual bestowal of the Gold Medal of the Society, as the reward of some astronomical work, had become so much a matter of course, that the Address of the President on the occasion was always expected, as one of the most interesting parts of the proceedings at the General Meeting. He (the Chairman) was sorry, on this account, to have to announce to the members that the Council had on this occasion deviated from their ordinary rule, and that the Medal had not been adjudged. It needed scarcely to be mentioned that this determination was not occasioned by any want of meritorious works which deserved such a reward. The Report which had been read would prove the contrary. In fact, so far was it from being the case that any stagnation or diminution of energy had taken place in the prosecution of Astronomy, that the Report of the progress of the science was unusually interesting. Almost every department had made a marked advance. In Sidereal Astronomy, while Krüger had found measurable parallaxes for two or three additional stars, the unwearied Goldschmidt had been successfully engaged on the system of *Sirius*, and had been rewarded with the discovery of some other minute companions. In the Solar System the advances in our knowledge were still more important. The necessity for a considerable augmentation of the Sun's Parallax had been established by so many different investigations, that it scarcely admits of further controversy, and there is little doubt that the correct value is now known within a very few hundreds of a second. The distance of *Mars* had been measured and delineated at the last opposition with an accuracy never before attempted. Mr. De La Rue, far from being content with the admirable results which he has obtained in Celestial Photography, has made additional and successful efforts for its further improvement. And finally,

the surface of the Sun has been the subject of study to various astronomers best fitted, by their intelligence, their sharp-sightedness, and their command of appropriate apparatus, to extend our knowledge of this marvellous body.

But, in the account of the progress of English Astronomy during the past year, it would be unpardonable not to give a prominent place to the publication of Mr. Carrington's book on the Solar Spots. It would be premature to eulogise this admirable production, the merits of which will no doubt at some future time be brought prominently before the Society; but he (the Chairman) could not avoid saying that he recognised in it the same careful elaboration and finish, the same attention to the minutest necessary details of observation and calculation, coupled with the broadest and most sagacious theoretical views, which had rendered the Red Hill Catalogue of Stars a classic amongst similar productions.

It was not, therefore, for lack of works worthy of reward that the Council have, in this instance, intermitted their usual practice; but, more properly it might be said, that they have been induced to do so on account of the very abundance of them; for only a few out of many have been mentioned in this very imperfect survey.

The frequency of discoveries in many branches of research, and the vigour with which these researches are carried on, make it probable that we are on the eve of obtaining still greater results; and the Council consider that they best regard the interests of Astronomy, as well as of the cultivators of the science, by deferring their judgment till the next steps have been gained, when they will be enabled, in the Address from the Chair, to illustrate better some particular point of the progress of the science.

It is hoped, then, that the disappointment naturally attendant on a departure from the usual rule will be allayed when these circumstances are taken into account; while at the same time the carefully drawn-up Report of the progress of Astronomy, which has been read, will be felt to have rendered an address on the Medal less necessary than at some other times.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

WARREN DE LA RUE, Esq. F.R.S.

Vice-Presidents:

Professor J. C. ADAMS, Esq. M.A. F.R.S.
G. B. AIRY, Esq. M.A. F.R.S. Astronomer Royal.
JOHN LEE, Esq. LL.D. F.R.S.
C. B. VIGNOLES, Esq. F.R.S.

Treasurer:

SAMUEL CHARLES WHITBREAD, Esq., F.R.S.

Secretaries:

RICHARD HODGSON, Esq.
Rev. C. PRITCHARD, M.A. F.R.S.

Foreign Secretary:

Admiral R. H. MANNERS.

Council:

R. C. CARRINGTON, Esq. F.R.S.
Professor CAYLEY, M.A. F.R.S.
EDWIN CLARK, Esq.
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JOHN RUSSELL HIND, Esq., F.R.S., Superintendent of the *Nautical Almanac.*
WILLIAM HUGGINS, Esq.
Rev. ROBERT MAIN, M.A. F.R.S. Radcliffe Observer.
Rev. Professor SELWYN, B.D.
Major-Gen. SHORTREDE.
JAMES SIMMS, Esq.
EDWARD J. STONE, Esq. M.A.
Lieut.-Col. A. STRANGE.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIV. *March* 11, 1864. No. 5.

WARREN DE LA RUE, Esq., President, in the Chair.

A List of New Double Stars discovered by the Rev. W. R. Dawes.

It seems to be desirable to direct attention to newly discovered double stars, that the question of their physical connection may be determined by observations instituted by such amateurs as possess instruments suitable for the work. I therefore beg to present to the Society the following list of such objects, containing those which have been met with by myself; for, though a few of them have been already published from time to time soon after their discovery, they have never been collected together so as to be readily referred to; and it is therefore very probable that they are little known, and have been but seldom observed. Most of them were casually picked up on nights too unfavourable for satisfactory measurements, and with no very high powers, seldom exceeding 300. As I have employed very little time in sweeping for such objects, it seems probable that an amateur observer possessing sufficiently powerful means, and disposed to devote himself steadily to it, might add materially to the number. And though it is very probable that by far the greater portion of the excessively unequal pairs are only *optically* double, yet the example of that

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On a Mode of Figuring Glass Specula, for the Newtonian Telescope. Rev. H. C. Key.

On the Movement of the Solar System in Space. Mr. Dunkin.

Observations of *Mars*, with Drawings. Mr. Lassell.

On the Visibility of Stars in the *Pleiades* to the unassisted Eye. The Astronomer Royal.

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On the Parallax of certain Stars. M. Auwers.

On the Movement of the Solar System in Space, deduced from the Proper Motion of 1167 Stars. Mr. Dunkin.

Sur une Equation pour le Calcul des Orbites Planetaires. M. De Gasparis.

On the Madras Observatory. Mr. Pogson.

Occultations observed at Highbury. Mr. Burr.

Apr. 10. On the Appearance of the Dusky Ring of *Saturn*. Mr. Carpenter.

Note on a Small Companion of *Procyon*. Mr. J. G. Barclay.

Observations of Comet I. 1863. &c. M. Romberg.

Determination of the Sun's Mean Horizontal Parallax, from a Comparison of corresponding Observations of N.P.D. of *Mars* and Stars, made at Greenwich and Williamstown during the Opposition of 1862. Mr. Stone.

Occultation of B.A.C. 4006. Capt. Noble.

May 9. Observations of Encke's Comet, at the Royal Observatory, Cape of Good Hope, No. I., from June 3, 1861, to June 23, 1861. Sir T. Maclear.

Ditto ditto No. II., from Dec. 23, 1861, to March 12, 1862. Sir T. Maclear.

Translation of part of Letter to the Astronomer Royal. M. Wolf

On the Discordance between Reflexional and Direct Observations in Zenith Distance. The Astronomer Royal.

On the Indications by Phenomena of Atmospheres to the Sun, Moon, and Planets. Prof. Challis.

On the Solution of some Problems in Nautical Astronomy. Mr. Hannyngton.

Observation of a Comet (III. 1863). Mr. Petty.

Further Observations upon Solar Spots. Rev. W. Howlett.

Occultations of Stars by the Moon, &c. Mr. Gorton.

Remarkable Phenomena attending the Lunar Occultation of χ *Cancri*. Rev. W. R. Dawes.

Observation of the Lunar Crater *Plato*. Rev. W. R. Dawes.

Observations of Comet II. and III. 1863. Mr. Hodgson.

Note on the Coefficient of Parallactic Equation in the Lunar Theory. Mr. Stone.

On a Portion of the *Mare Imbrium*. Mr. Birt.

Elements of Comets II. and III. 1863. M. Romberg.

Jupiter's Satellites, &c., Are they Visible to the naked Eye? Mr. C. Mason.

Occultation observed at Highbury. Mr. Burr.

On a Solar Eclipse recorded in Ancient Chinese History. Mr. Williams.

Observation of a Solar Facula and Occultation of χ *Cancri*. Capt. Noble.

Observations of the Planet *Mars*. Mr. Lockyer.

Observations of *Astrea* and of the Solar Eclipse of May 17, 1863. Mr. Hartnup.

On the Lunar Eclipse of June 1. Prof. C. P. Smyth.

On the Eclipse of the Moon, June 1, 1863. Mr. H. S. Ellis.

Occultation of κ *Cancri*. Sig. J. Vertu.

On his Apparatus for producing Uniform Motion. M. Foucault.

On the Constant of the Moon's Equatoreal Parallax. Mr. H. Breen.

On the Companions of *Sirius*. M. Goldschmidt.

Nov. 13. Statutes of a new Astronomical Society founded at Heidelburg. M. Zech.

Calculation of the Sun's Parallax from the Lunar Theory. Translated by W. T. Lynn, Esq. M. Hansen.

On the Luminous Band round the Moon during Partial Eclipses of the Sun. The Astronomer Royal.

Inferior Conjunction of *Venus*. Mr. Prince.

results for longitude with the lunar distances corrected and uncorrected for the variations in the readings of the barometer and thermometer. In the reduction of Capt. Speke's astronomical observations, I was obliged to assume these readings, in consequence of no recorded barometric readings having been placed in my hands; but, fortunately, scarcely any ill effect arose from this circumstance, as I found subsequently from an examination of some observations of the temperature of the boiling point of water made during the route, that my assumed readings differed very little from the truth.

From the mean of a series of sextant observations made at Kazé by Capt. Speke, on 1861, February 28, at $20^h\ 25^m\ 34^s$ local time, it was found that the angular distance, at that time, between the Sun and Moon, when cleared from refraction and parallax, was 123° 10′ 28″ corrected, and 123° 11′ 4″ uncorrected. These values give for longitude:—

	h m s		° ′ ″
Corrected	2 12 4 E.	=	33 1 0 E.
Uncorrected	2 13 8	=	33 17 0
Difference	1 4	=	16 0

Again, in 1862, December 12, at Faloro, at $20^h\ 37^m\ 27^s$ local time, the angular distance between the Sun and Moon was found to be 103° 39′ 1″ corrected, and 103° 39′ 25″ uncorrected; the resulting values for longitude being:—

	h m s		° ′ ″
Corrected	2 7 25 E.	=	31 51 15 E.
Uncorrected	2 8 15	=	32 3 45
Difference	0 50	=	12 30

I think these differences are sufficiently important to attract the attention of those who may in future be called upon to make or reduce similar astronomical observations for geographical purposes. They also justify me in laying the subject before the Society. I would, therefore, most strongly recommend that future travellers should be specially instructed to record, not only the readings of the chronometer and sextant, but also the readings of the barometer and thermometer *at least once* during each series of observations. If they will faithfully do this, they will greatly relieve the mind of the computer, and also (which is of more consequence) increase the value of their observations two-fold.

22 *Royal Circus Street, Greenwich,*
March 8, 1864.

On the Estimated Number of Luminous Particles contained within a defined space on the Sun's Disk. By Edwin Dunkin, Esq.

It may be interesting to record in the *Monthly Notices* of the Society the result of a scrutiny of the solar disk, made on March 10 with the telescope of the Altazimuth, at the Royal Observatory. The small luminous particles identical with the "willow-leaves" of Mr. Nasmyth were seen at intervals distinctly over the whole surface of the Sun, the power used being about 100; and from the peculiar system of wires inserted in the eye-piece of the telescope, I was enabled to form a rough estimation of the number of these particles contained within a definite space.

In the centre of the system of horizontal wires several clear spaces are formed by the intersection of the two central horizontal wires and the six vertical wires. These spaces are nearly square, but they can be more accurately represented by stating that the angular distance between the wires is 56″ in a vertical direction, and 48″ in a horizontal direction. Now the number of particles enclosed at one time within these spaces was estimated to be about three hundred, say, about twenty in one direction and fifteen in the other, and scattered equally about. Mr. Carpenter, who was with me at the time, sensibly confirmed these numbers. As a deduction from these observations, I consider that the average length of these particles is about two seconds, though there are some larger and many smaller.

The aperture of the telescope is about 3¾ inches.

With regard to the form of these objects, as exhibited by the Great Equatoreal, I think the "willow-leaf" is very exceptional, and that Mr. Stone's definition, like "grains of rice," is much more applicable. If we take a handful of common grocer's rice, we shall find that the forms are various, some elongated, some round, some broken off sharply. Now I have seen samples of each of these forms in the solar rice, and I know of no better illustration of these phenomena than that exhibited to the Society by Mr. Stone, in which a small quantity of rice was dropped gently and promiscuously on paper covered with an adhesive mixture. The appearance given by this simple experiment gives a fair idea of what can be seen on the solar disk when the observer knows beforehand what kind of object he is in search of.

The "grains of rice" were also visible on the same morning through the telescope of the North Equatoreal. The aperture of the object-glass of this telescope is 4·1 inches.

Greenwich, March 11, 1864.

Note.—On an examination of the Sun's disk with the

Altazimuth, on March 16, I thought that the luminous particles contained in the nearly square spaces appeared to be more thinly scattered than when noticed on March 10. On this second occasion I roughly estimated the number to be about 200.

March 23, 1864.

Mr. Nasmyth's Willow-Leaves. By E. J. Stone, Esq.

The subject of Mr. Nasmyth's "willow-leaves" having been lately brought before this Society, my attention was drawn to the subject. On 1864, Jan. 26, I turned the Great Equatoreal of the Greenwich Observatory on the Sun for the express purpose of looking for these objects. I then distinctly saw the general luminous surface of the Sun irregularly scattered over with bright rice-like particles: these particles were most numerous about the parts of the disk which presented the well-known mottled appearance. I consider the mottled appearance to be produced by the interlacing of these bright particles. Their general shape appeared to me to be far more nearly that of rice than of willow-leaves. I think the description of them as bright rice-like particles is, at least, sufficiently accurate to enable any observer to know exactly what he has to look for, and in this case there appears to be no great difficulty in seeing them. On the day I first saw them, Jan. 26, I called Mr. Dunkin's attention to these particles, and he saw them. They have since been seen on several occasions by myself, Mr. Dunkin, Mr. Ellis, and Mr. Carpenter. On March 10 Mr. Dunkin and Mr. Carpenter saw them with the Shuckburgh telescope of $4\frac{1}{2}$ inches aperture.

Power used, 50·100 Huyghen diagonal reflector.

On early Observations of Uranus by Bradley. By Hugh Breen, Esq.

In a recent examination of Dr. Bradley's unpublished *Greenwich Observations* I have discovered the two following observations of *Uranus*. The first was observed as a star of the sixth magnitude by the transit instrument, and the second by the quadrant:—

New Style.		G.M.S.T.	R.A.	N.P.D.
		d h m s	h m s	° ′
1748	October	21 7 6 18·4	21 4 37·93	107 29
1750	September	13 10 8 57·8	21 40 0·23	104 48

The right ascensions are very accurate.

It had been assumed that the north polar distance in 1750, Sept. 13, was identical with *α Capricorni*, with which it was observed; but some calculations which I have made show me that it was about 5′ greater than that star. I have given the north polar distance accordingly. It was noted as being, on this occasion, of the sixth magnitude.

I intend to make an accurate comparison with Bouvard's Tables, which I hope will be of some use in any future investigations.

Retreat, Lewisham, Feb. 8, 1864.

Mr. Henry Cooper Key, in a letter dated Stretton Rectory, Hereford, Nov. 11, 1863, communicated to the Society a description, illustrated by a drawing, of his stand for a reflecting telescope: the drawing is somewhat too complex for representation in the reduced scale of a woodcut, and without it the description would not be intelligible.

Comet VI. 1863.

The Comet was discovered (see *Ast. Nach.* No. 1456) by Prof. Respighi at Bologna on the 28th Dec. 1863; the original observations being as follows:—

	Bol. M.T.	R.A.	Decl.
	h m s	h m s	° ′ ″
1863, Dec. 28	6 43 4	18 49 24·80	+25 57 33·7
	18 11 2	18 50 1·76	+26 13 2·2

A set of parabolic elements was calculated by Dr. Weisse (*Ast. Nach.* No. 1458), and it was remarked by him that the path closely resembles that of the Comet of 1810: this gives a period of 53½ years; and then, reckoning back six revolutions, it appears that there was, in 1490, a Comet, the path of which also resembles that of the present Comet. But the observations of the present year seem to be better satisfied by an elliptic orbit with a period of about 108 years (see *Ast. Nach.* No. 1465), where Dr. Michez, of the Observatory of Padua, gives the following set of elements from observations, 1864, Jan. 7 and 23, and Feb. 8:—

Perihelion Passage, 1863, Dec. 27035745 G.M.T.

Log Perihelion Dist.	q	=	9·8854860
,, Mean Daily Motion	μ''	=	1513555 2
,, Semi-axis Major	a	=	1·3576339
,, Semi-axis Minor	b	=	0·7683839

Long. Ascending Node	Ω	=	304°	52′	15″·5	Mean Equinox 1864·0
,, Perihelion	π	=	59	40	8·3	
Inclination	i	=	63	56	28·8	
Angle of Excentricity	φ	=	75	4	45·1	
Period	T	=	1087·76			

Motion direct.

The error for the mean observation being thus:—

Long. O − C = + 1″·5 Lat. O − C = − 0″·5

The identity of the Comet with that of 1810 is thus doubtful.

Minor Planet (70) *Eurynome.*

Discovered by Mr. Watson, Director of the Ann Arbor Observatory, 14th Sept. 1863.

The following set of elements, calculated by Mr. W. A. Royers, from the *Washington Observations*, to Dec. 14, from twelve Ann Arbor observations, and from thirty-five observations by himself at the Alfred University Observatory, from which were computed normal places for 1863, Sept. 24, Oct. 18 and 30, Nov. 19, and Dec. 5, are given, *Ast. Nach.* No. 1465:—

Epoch, 1863, Sept. 19 G.M.T.

M	=	334°	43′	16″·9
π	=	44	13	28·7
Ω	=	206	40	54·7
i	=	4	36	49·1
φ	=	11	14	28·9
log a	=	0·3880180		
μ	=	928″·92424		

In the *Comptes Rendus*, 7th March, 1864, there is contained a paper by M. Faye, "On a New Method proposed by M. de Littrow, for Determining the Time and the Longitude at Sea." The method was tried on the voyage of circumnavigation of the Austrian frigate, the Novara, and after trial was adopted by the Commander Admiral von Wüllerstorf, as part of the daily routine of the vessel. These results attracted the attention of M. Faye, and M. de Littrow having requested him to make the method known in France, he accordingly communicated it to the Academy.

The method consists in the determination of the time by two circummeridional observations of the Sun, preserving at the same time the observation of the true noon for the latitude. The two altitudes may be taken at pleasure on the same side or on opposite sides of the meridian: the interval of time is arbitrary, varying, according to the circumstances, from 5 to 30 or 40 minutes; and as the calculation takes only 5 minutes, the navigator may in half-an-hour take his observations, and effect all the calculations necessary for finding at once the longitude and the latitude. It is to be observed that the two determinations are independent of each other, and that thus the method is not to be considered as merely a bold extension of the method of Douwes, one of whose fundamental equations is made use of. So far from replacing the habitual meridian observations, the new method is an aid to it by giving beforehand an approximate determination of the time of culmination.

The new idea depends on the remark that, when, as at sea, a scrupulous accuracy is not required, the circummeridional altitudes of the Sun may be used for determining the time Using h and h' to denote the altitudes taken at the times t and t' of the chronometer; T and T′ the corresponding hour-angles; φ the latitude, and δ the declination: then, in order to determine the mean $\frac{1}{2}$ (T + T′) of the unknown hour-angles, there is the relation already familiar to navigators

$$\sin \frac{1}{2}(T + T') = \frac{\sin \frac{1}{2}(h' - h)}{\sin \frac{1}{2}(T - T)} \cdot \frac{\cos \frac{1}{2}(h' + h)}{\cos \varphi \cos \delta};$$

but which is only applied by them to observations, of which one, at least, is taken as far as possible from the meridian.

As an example, M. Faye gives as follows:—

Suppose that, for a place 20° north latitude on the 25th August, 1864, there are taken the following observations about 30 and 15 minutes before true noon:—

$$\text{At } \begin{matrix} 3^h\ 31^m\ 45^s \\ 3\ \ 46\ \ 45 \end{matrix} \text{ Paris Mean Time } \left.\begin{matrix} h = 78^\circ\ 6'\ 19'' \\ h' = 79\ 52\ 49 \end{matrix}\right\} \delta = 10^\circ\ 33'$$

thence

$$\tfrac{1}{2}(T - T') = 1^\circ\ 52'\ 30'' \qquad \tfrac{1}{2}(t + t') = 3^h\ 39^m\ 15^s$$
$$\tfrac{1}{2}(h' - h) = 0\ \ 53\ \ 15 \qquad \tfrac{1}{2}(h' + h) = 78^\circ\ 59'\ 34''$$

and

log sec φ	0·02701
log sec δ	0·00740
log cos $\frac{1}{2}(h' + h)$	9·28088
log cosec $\frac{1}{2}(T - T')$	1·48520
log sin $\frac{1}{2}(h' - h)$	8·19000
log sin $\frac{1}{2}(T + T')$	8·99040

$\frac{1}{2}(T + T')$ . . . . $5^\circ\ 36'\ 52'' = 0^h\ 22^m\ 27^s$ E.

whence

	h m s
True Local Time	23 37 33
Equation of Time	1 45
Mean Local Time	23 39 18
Paris Time	3 39 15
Longitude, West	3 59 57

and for the ordinary meridian observation by the chronometer, the instant of true noon will be $4^h\ 1^m\ 42^s$.

	° ′ ″
Observed Altitude	80 32 26
Decl. + 90°	100 32 36
φ, Concluded Latitude	20 0 10

For estimating the probable error of the determination, M. Faye forms the equation,

$$\frac{d \cdot \frac{1}{2}(T + T')}{\tan \frac{1}{2}(T + T')} = \frac{d \cdot \frac{1}{2}(h' - h)}{\tan \frac{1}{2}(h' - h)} - \frac{d \cdot \frac{1}{2}(h + h')}{\cot \frac{1}{2}(h' + h)} + \frac{d\varphi}{\cot \varphi},$$

which, with the foregoing numerical values, gives, if $d\,L = \frac{1}{15}\, d \cdot \frac{1}{2}(T + T')$, the error of longitude expressed in time,

$$d\,L = 0^s{\cdot}422\, d \cdot \tfrac{1}{2}(h' - h) - 0^s{\cdot}034\, d \cdot \tfrac{1}{2}(h' + h) + 0^s{\cdot}0024\, d\varphi;$$

and thence, taking as probable amounts for the errors, $d\varphi = 10'$, $d\,\frac{1}{2}(h' + h) = 1'$, $d\,\frac{1}{2}(h' - h) = 13''$, the resulting portions of the error $d\,L$ are $1^s{\cdot}44$, $2^s{\cdot}04$, and $2^s{\cdot}74$, and the final error to be feared is

$$\sqrt{\overline{2{\cdot}74}^2 + \overline{2{\cdot}04}^2 + \overline{1{\cdot}44}^2} = \pm\, 3^s{\cdot}70;$$

that is to say, about an error of 1 nautical mile. But the example is a favourable one for the method; and it is not always that so much precision would be obtained; for instance, if the observations had been made in winter, instead of summer, the 20th February instead of 25th August, then, with the same errors of observation, the resulting error in the longitude would be $\pm\ 7^s{\cdot}70$, or about 2 nautical miles. He concludes that the method, as above explained, furnishes excellent results for culminations near the zenith; in the contrary case the precision diminishes, but it is easy to bring it up to the required accuracy by increasing the interval between the circummeridional observations: the only exception is for the regions of

the globe and the circumstances of the sea, for which all possible methods are in defect.

M. Faye remarks, that the method might be equally useful on *terra firma* as at sea: for travellers, as well as sailors, it would be useful to have a convenient method of determining daily their latitude and longitude by observations concentrated at a single epoch of the day, about noon; and he suggests the employment in this case of a correction for the variation of the Sun's declination, as follows:—

Differentiating the equation

$$\sin h = \sin \varphi \sin \delta + \cos \varphi \cos \delta \cos T$$

in regard to h and δ, and in the result replacing cos T by unity, we have

$$\frac{d\,h}{d\,\delta} = \frac{\cos h_{,}}{\cos h},$$

where $h_{,}$ is the meridian altitude of the Sun. This ratio being sensibly = 1, except in the case of a culmination very near the zenith, $d\,\delta$ may be taken for $d\,h$, which gives the rule. "In order to take account of the change of declination in the interval of the two measures, subtract it from the second altitude when the change tends to elevate the Sun; add it in the contrary case."

The rule fails for a culmination near the zenith, but the correction is then of less importance, on account of the smallness of the coefficient of $d\,\frac{1}{2}\,(h' - h)$ in the expression of $d\,L$; the correction may however be calculated from the equation

$$d\,h = d\,\delta \cos S,$$

where S is the angle at the Sun, given by the equation

$$\frac{\sin S}{\sin \varphi} = \frac{\sin T}{\cos h}.$$

It is easy, also, to correct the error introduced by the employment of an estimated value of the latitude, and so in the expression for $d\,L$ to get rid of the term depending on $d\,\varphi$. And, lastly, if by any accident the meridian observation is wanting, the ordinary reduction to the meridian may be applied to one of the observed altitudes, so as to obtain with sufficient accuracy the meridian altitude, and thence the latitude.

Note by the Astronomer Royal on a Passage in Captain Jacob's "Measures of Jupiter and his Satellites taken with the Madras Equatoreal."

In the very admirable paper of the late Captain Jacob, printed in vol. xxviii. of the Royal Astronomical Society's *Memoirs*, in the section "Mass of *Jupiter*," page 110, when describing the process of reducing the measures of elongation of *Jupiter's* satellite IV., which is done by use of my tables in vol. vi. of the *Memoirs*, page 91, Captain Jacob remarks, "My reason for not adopting Mr. Airy's epoch was, that it was not apparent whether the light-equation was included in it or not."

It is curious that Captain Jacob should not have noticed the explicit statement by myself, which follows the cited tables at an interval of a single page. In page 92 of vol. vi. I state, "The times in the table in Section I. are diminished by the light-equation corresponding to 20″·5 constant of aberration, and the geocentric place of the satellite is calculated &c." Before commencing that calculation, the times obviously are not so diminished, or the light-equation is not included.

I have referred to the original calculations, and I find that all are conducted in conformity with this statement.

March 10, 1864.

A paper has been received from Mr. H. P. Finlayson, dated Dover, March 4th, 1864, noticing the following relation:—

Period of rotation of the Earth on its axis
: Periodic time of Moon round the Earth
: : Period of rotation of Sun on its axis
: Periodic time of *Mars* round the Sun,

viz.

1 : 27·32166 : : 25·188 : 686·979645,

which, as the author imagines, probably subsists with an absolute mathematical precision, and from which he deduces various results.

RECENT PUBLICATION.

Practical and Spherical Astronomy, for the use chiefly of Students in the Universities. By the Rev. R. Main, Radcliffe Observer at Oxford. Cambridge and London, 1863.

Mr. Main's excellent work has been already referred to in the Report of the Council to the last Annual Meeting of the Society. The author states that his object has been to include both in the practical and theoretical portions of the volume all the mathematical processes which will enable the reader to understand the operations of a modern Observatory, furnished with the ordinary meridional and extra-meridional instruments, so that he might readily acquire, if called upon to do so, by actual practice in an observatory, the additional and more minute details which occur in the making and reducing of observations; while, therefore, in the description of the instruments, the details of their construction are not minutely entered into, on the other hand, the author hopes that there is scarcely a process used at Greenwich, or our other great English Observatories, which is left unexplained, and that a student will be enabled, by the help of the treatise and by suitable tables, to reduce every class of observations which will be found in their annals, or to examine the accuracy of their results. The especial value of Mr. Main's treatise consists in this happy combination of its mathematical and practical bearing, leading the student right up to the Observatory, and furnishing him with the very best methods of applying what, up to that point, must otherwise be in general half comprehended theory. The execution is such as might be expected from the position and character of the author, and it is almost needless to say that the work may be relied on as furnishing an accurate representation of the present practice of astronomy.

Curiously enough, the author has omitted the only method of finding the azimuth error of a transit, which is really and readily applicable for a small instrument, viz., by observing a high and a low star: a method invaluable where the clock is not absolutely first-rate, and where, as in all portable instruments, the aperture of the telescope is small.

There will be read at the ensuing Meeting of the Society, on the 8th April, a paper by A. Herschel, Esq. "On the Connexion between the number of Meteorolites, as visible at any place, and their planetary nature."

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIV. *April* 8, 1864. No. 6.

WARREN DE LA RUE, Esq., President, in the Chair.

Charles Todd, Esq., Director of the Observatory at Adelaide;
Henry Syme Redpath, Esq., Sydenham; and
Frederick Hungerford Bowman, Esq., Halifax,
were balloted for and duly elected Fellows of the Society.

It was announced that a List of the Fellows, corrected up to the 31st December, 1863, will shortly be distributed. There is a difficulty in getting it absolutely correct; and the Fellows are requested to make known to the Assistant Secretary any corrections that are necessary.

State of Meteoric Science. By A. S. Herschel, Esq.

(*Communicated by the Rev. C. Pritchard.*)

It is a saying of Arago, founded originally upon observation and confirmed by constant experience in later years, that the Earth encounters more shooting-stars in going from aphelion to perihelion than in going from perihelion to aphelion.

In point of fact, between the north latitudes of 49°·5 and 54°·2, Dr. Julius Schmidt, the indefatigable Director of the Observatory at Athens, observed, during eight years, from 1843 to 1850, on an average 470 meteors in every year. His observations were distributed in the several months, according to the following average of the entire series:—

Anomalistic Year.	Month.	Shooting-Stars.	Total Shooting-Stars.
Aphelion to Perihelion	July	49	400
	August	188	
	September	41	
	October	37	
	November	54	
	December	31	
Perihelion to Aphelion	January	17	70
	February	5	
	March	11	
	April	11	
	May	12	
	June	14	
		Total	470

Supposing, with Arago, that shooting-stars are meteorites or particles describing planetary orbits in all directions round the Sun, no one of these meteorites can overtake the Earth, if the native velocity of all is less than the planetary motion of the Earth itself; on the contrary, the Earth will overtake, in a greater or less degree, the whole of those which it meets, if we suppose its velocity to be greater than that of every individual shooting-star. Even upon more general assumptions, however, of the orbital velocities of meteorites, it follows that a more or less powerfully determined current of falling-stars must arrive upon the Earth, owing to its planetary motion, from a constantly prevailing direction ninety degrees in longitude behind the Sun.

On this account a season of frequency of aerolites, shooting-stars, and bolides, must be expected to succeed, in all latitudes, three months later than the summer season of the Sun; but, on the other hand, a dearth of meteors, in the spring, one quarter of a year later than mid-winter. In general, and in all latitudes, the meteoric seasons, or seasons of meteoric frequency, must strictly follow the tropical seasons, and three months later in the year. Thus, in the Earth's northern hemisphere the northern pole remains directed to the Sun from the equinox of March until that of September, and to the course of meteors from the solstice of June to the solstice of December. The

greatest frequency of the meteorites will fall about the equinox of autumn, in September and October. This most nearly agrees with the European observations. The meteoric season of Arago may, therefore, be drawn as a consequence from his planetary hypothesis, if it be permitted to change the limits which he assigns to it by a small quantity, namely, from the Earth's apsides to its solstices in its orbit.

The same fact, which appears strongly marked with regard to shooting-stars in the eight-year summary of Dr. Schmidt, is found repeated in a striking manner in the existing Northern Catalogues of *Star-showers*, *Fire-balls*, and *Aeroliths*. The following references may be taken as examples:—

Appearances.	Number, July to December.	Number, January to June.
Star-showers, from 1800 B.C. In M. Quetelet's Catalogue (*Physique du Globe*, 1861)	72	28
Aerolitic Meteors, from the Christian Era. In Mr. Greg's Catalogue (*Brit. Ass. Report*, 1860)	216	186½
Large and small *Fire-balls*. In same Catalogue (*Ibid.*)	843	553

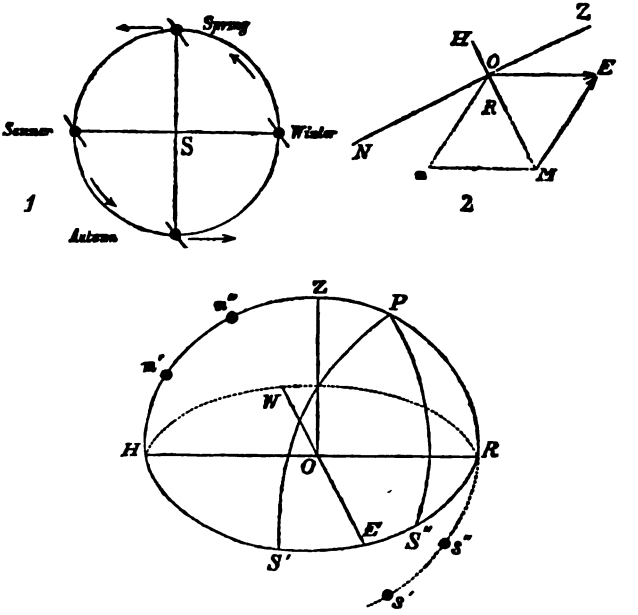
The time appears therefore at length to have arrived when observations of meteors in the southern hemisphere may greatly contribute to establish the *planetary* theory of meteorites. Should the same sequences of meteoric seasons, after the tropical seasons, be observed in the Southern hemisphere which are followed in the north, the record of meteors in Southern Observatories will become at once a confirmation of the planetary motion of the Earth, and a proof of the correctness of the existing theory of shooting-stars. A sufficient number of data can only be collected in the course of time; but the observations of shooting-stars at Melbourne, in South Australia, already preserved for some years, in great numbers, by Dr. Neumayer, appear actually calculated to give to this question, before long, a satisfactory solution.

March 1, 1864.

After the reading of the preceding paper, Mr. Pritchard, according to a previous arrangement, addressed to the Meeting certain remarks, of which the following is a summary:—

The great merit of Mr. Herschel's communication consists in this, that he has shown how certain principles which have already been applied to account for the increased number of

meteorolites visible in the later hours over those visible during the earlier hours of the night grasp and explain also why in northern latitudes more meteors appear at and about our autumn than at or about our spring, and in general more meteors from summer through autumn to winter, than from winter through spring to summer.



In the seventeenth volume of the *Monthly Notices*, p. 148, 8, there is a very short but valuable paper on this subject by Mr. Bompas, which, on account of its extremely condensed form, has probably met with far less attention than it deserves. The author of the paper thus inopportunely abridged shows how, by attributing to meteors an average velocity double that of the Earth in its orbit, nearly two-thirds of their number would have an apparent motion opposed to the motion of the Earth, with a gradual increase of frequency during the night from 6 A.M. to 6 P.M. This phenomenon Mr. Bompas finds completely in accordance with the extensive observations of M. Coulvier Gravier. Mr. Herschel has extended the application of the principle of Mr. Bompas to *annual seasons* of meteoric frequency; and in a communication yet to be made to the Society he will show how certain formulæ of calculation

which he has investigated appear to represent with considerable accuracy observations of a trustworthy character.

It may be of advantage to the Members for Mr. Pritchard to place this very interesting though but little-understood subject in the simple point of view in which it has occurred to himself. The hypothesis made regarding shooting-stars is that they move in orbits round the sun, exhibiting (so far as is yet known) no preference whatever either with regard to eccentricity, to inclination to the ecliptic, or to direct or retrograde motion. Neither is reference now made to any want of uniformity in their distribution in the intrastellar region. Our object is to show that this hypothetical uniformity of distribution, combined with the direction and amount of the Earth's motion, will have a very sensible effect on the number of meteors actually visible at a given place, at a given hour of the night, as explained (in a somewhat different way) by Mr. Bompas, and at a given season of the year, as extended by Mr. Herschel.

For the purposes of illustration, suppose H O R (fig. 2) to represent a flat umbrella, of which N O is the stick; suppose, also, rain to fall upon it equally, and in all directions: then, if the umbrella be at rest, as much rain will fall on its front, looking from Z, as on its back, from N.

But now suppose the umbrella itself has a motion from O to E in a given time, and, for simplicity of first conception, let O E represent also the uniform velocity of the rain: very much more rain will now fall on the front of H O R, and much less on the back of H O R, than before. In fact, if O *m* be taken = O E, and the angle *m* O R be made = R O E, and the parallelogram *m* E be completed, then a raindrop, of which *m*'s real path is M E, would by the motion of H O R just graze along the front surface of H O R in the direction M R O, when it arrives at E. Moreover all the rain which at the beginning of the motion was moving within the angle *m* O R, and which would have fallen on the back of H O R at rest, will now fall on the front of H O R, if in motion.

The application of this hypothetical case to that of meteors is obvious. H O R now represents the horizon, Z the zenith of an observer, and O E the direction and magnitude of the Earth's orbital motion. The Earth's diurnal motion of rotation is comparatively too small to be taken into the account for our present purposes. So long, then, as O E, the direction of the earth's orbital motion, is in front of the horizon of an observer, there will thereby occur to him an additional flow (and partial combustion) of meteors against the Earth's atmosphere above him; and this increased flow will become the greater, as the angle R O E becomes greater. If O E be below H O R, then the number of visible meteors will thereby be diminished.

In spring, for instance (fig. 1),* the Earth is moving in the same direction as the observer had seen the Sun in winter, viz., *w* S. To ascertain, therefore, in what direction O E (fig. 2) the point O is moving at or about the vernal equinox, he need simply ask the question, where was the Sun when its right ascension was six hours less, and with the declination belonging to it three months before the time of the observation? At the vernal equinox, and at sunset, the Sun is setting in the west, W (fig. 3), at a place O, of which H W R E is the horizon, and H Z P R s'' s' the meridian.

For the place of a Sun having 90°, or six hours less of right ascension, and the declination of the mid-winter Sun, we must take (fig. 3) P R s'' s' = the ☉'s mid-winter N.P.D. = 90° + 23° 30′, and then O s' will be the direction of the Earth's motion, *i. e.* very nearly vertically downwards at sunset at the vernal equinox. In this case, and at this time, O E (fig. 2) lies at the back of H O R, and the number of visible meteors will thus be greatly diminished by the Earth's motion.

In London, about eight hours after the sunset at W, s' will reach the horizon at S′; *i. e.* about two o'clock in the morning the Earth will be moving in O S′ (fig. 3), in a horizontal direction; in other words, O E (fig. 2) will, about 2 A.M., be horizontal. About four hours after, *i. e.* at 6 A M., S′ will be on the meridian at *m* (fig. 3), and the angle R O E (fig. 2) will be at its greatest magnitude, viz. the altitude, which the Sun attains to at mid-winter, *i. e.* about 15°.

Again, at the autumnal equinox, a very slight consideration of the figures 1, 2, 3, will show that the Earth is then moving in the direction in which the Sun had been seen at midsummer. At 6 P.M. the Sun is setting at W, and the observer O (fig. 3) is moving downwards in the direction O s'', where Ps'', ☉'s midsummer N.P.D., = 90° − 23° 30′. About four hours later (see *Moore's Almanac*), *i. e.* at 10 P.M., the observer will be moving horizontally in O S″; and then the angle R O E (fig. 2) will continue to increase until about eight hours after 10 P.M. (*i. e.* at sunrise), when the motion of the observer will be in O m'' (fig. 3); the angle R O E (fig. 2) being equal to the altitude of the Sun at mid-summer, viz. about 62°. There will, therefore, in a northern latitude, in clear nights, be more shooting stars seen near the autumnal equinox than near the vernal, partly because O E is above the horizon during more hours of the night than at the vernal equinox, and partly because the direction of the Earth's orbital motion O E makes a considerably greater angle with the horizon H O R in autumn

* Fig. 1 is inadvertently lettered for the seasons in a southern latitude. It seemed scarcely worth while to correct it, especially as it may assist in showing how meteoric frequency follows the same *seasons*, both north and south of the equator.

than in spring. It follows also that at all seasons more meteors will be visible in an hour towards sunrise than in an hour towards sunset.

In a similar manner the same figures show distinctly what occurs at the summer and winter solstices; but a map of the stars, the *Nautical Almanac*, or even *Moore's Almanac*, together with the consideration of figs. 1, 2, 3, will be sufficient to enable an amateur to direct his observations of these strange meteoric bodies which swarm amidst the heavens. Such appears to be one general case of meteoric phenomena; but not unfrequently ephemeral swarms of meteors follow tracks emanating from a common centre, at capricious distances from the apex of the Earth's motion. For our knowledge of these we are at present mainly indebted to Mr. Grey's observations; but with regard to Mr. Herschel's ingenious extension of Mr. Bompas's principle enunciated in these remarks, the *experimentum crucis* will be, observations yet to be made in southern latitudes. And here it may be not alone pardonable, but even dutiful, to express our gratification that our knowledge of celestial phenomena is thus in progress of advancement through the successful labours of one bearing the name of Herschel. Rarely, indeed, is it accorded that intellectual acumen descends to the third generation.

It is to amateurs in astronomy especially that we must look for assistance in this interesting branch of celestial mechanics: these mineral fragments, these celestial rockets, this fiery dust from the lathe of the Omnipotent Worker, will furnish to him the correlative to that which the naturalist so fondly traces in the organic regions of creation—all space teeming with life—beauty, order, scattered on all sides with a lavish hand—yet everywhere, and all things, amenable to the control of law.—[C. P.]

Extract of a Letter from the Rev. W. R. Dawes to the Rev. C. Pritchard on the subject of the alleged recent Discoveries relative to the "Willow-leaved" Structure of the Sun's Photosphere.

"When Mr. Stone has presented his observations, as I suppose he will, to the Royal Astronomical Society, I may have more to say on this subject; but at present I will merely state that the observations of Messrs. Stone and Dunkin have *landed them precisely where I was sixteen years ago!* Soon after I had constructed a *transparent diagonal* on Sir John Herschel's principle (viz. *in the beginning of the year* 1848), and was delighting in being able by its means to see the whole disk of the Sun with a power of 65 on my Munich 6½-inch with its whole aperture in use, I was struck with the

appearance of bright particles scattered almost all over the Sun, which I compared to *excessively minute fragments of porcelain*—not, however, all of the same shape or size. Now here are undoubtedly the '*rice-grains*' *of* 1864; but, being even then *observationally* rather an *old bird* (more than thirty years old as an observer), I was not to be caught with *rice-grains*, but set to work by every means to ascertain what they really were. I had seen too much of optical illusions to announce any appearance, however distinct, *as a new discovery*, before it had stood the test of the most careful scrutiny under the best circumstances, and with the highest magnifying powers. It was nearly *four years* before I was completely satisfied on the subject; it was not, in fact, till I had completed the construction of my new solar eye-piece, which enabled me to place any portion of the Sun's surface in a small field, and to examine the identical objects with every variety of power, and under circumstances fit for the use of 400 to 600 with advantage. I thus arrived at the decided conviction that these brilliant objects were merely *different conditions of the surface of the comparatively large luminous clouds themselves*—ridges, waves, hills, knolls, or whatever else they might be called—differing in *form*, in *brilliancy*, and probably in *elevation*, and bearing something of the same proportion to the individual luminous clouds that the masses of bright *faculæ*, as seen near the Sun's edge, bear to the whole disk of the Sun.

"As I arrived at this conclusion nearly twelve years ago, and as all my subsequent observations have only confirmed my conviction of its correctness, I fear that *further looking* is not likely to alter it; and I am presumptuous enough to think that my own view of the matter arises, not from my having looked so little, or requiring to 'look again,' but from the fact that I have probably looked *much more* than any of the observers who have lately come forward on the subject."

The foregoing communication from the Rev. W. R. Dawes, [illegible], on the subject of willow-leaves, was read at the Meeting.

Mr. Pritchard remarked that the things asserted to have [illegible] been seen by Mr. Nasmyth, the President, and the observers [illegible] at Greenwich, were detached particles, nearly of a shape and [illegible] nearly of a size, spread nearly all over the Sun, the inter-[illegible]stices between these giving rise to its mottled appearance; [illegible] Mr. Dawes details, in his letter, what he has seen during [illegible] years of scrutiny, aided by his solar eye-piece. There is [illegible] a question whether the effect upon the eye, of looking through [illegible] a very minute aperture, gives the observer the real appearance, [illegible], or whether diffraction comes into play, to an inconvenient [illegible] extent. The other observers have seen the whole Sun's disk at [illegible]

once. Mr. Dawes asserts that these little bright spots are not distinct entities, but are merely distinguishable brightnesses—parts of the same thing, which happen to be brighter than the other parts.

Mr. Huggins alluded to recent observations made when the atmosphere was peculiarly fine, so that he was able to use a much higher power (200) than usual. He has been long accustomed to see these bright particles which Mr. Stone has compared to rice, and which resemble rice very much when viewed with insufficient magnifying power on the telescope. But, on examining them with a higher power, they appear to him not to be uniform in size, and of almost every imaginable shape, though generally the two diameters are nearly equal. The point at issue is the relation which the bright particles bear to the mottling of the Sun's disk. Mr. Huggins perceived them most distinctly with the higher power on the darker portion of the Sun's mottling, and in every case separated, with a dark portion between them; they certainly do not overlap each other. Mr. Huggins has seen much larger masses of luminous matter, which, under an exceedingly good definition, are not broken up at all into minute particles; but, with a power of 50 or 100, these minute distinctions could scarcely be perceived; they are quite distinguished from the bright luminous streaks which Mr. Dawes has most happily compared to straws. They are also remarkable as ending abruptly or square; evidently not ending in a point where they project over the dark portions of the spot. It would certainly seem that the existence, over the whole surface of the Sun, of bodies having anything at all of the shape of willow-leaves, is incompatible with the appearance of the Sun on the occasions to which he referred. Mr. Huggins' observations were made with a diagonal glass reflector. In Mr. Dawes' eye-piece there is a narrow ring of diffracted light immediately within the margin of the opening; in the centre of the field, however, admirable definition is obtained.

The Rev. Mr. Howlett stated that, one day, during the autumn, he saw the well-known mottling on the Sun's surface; the comparative magnitudes of the bright particles varied from 5 to 9 seconds. He described them as crystalline flakes.

Capt. Noble, with a 4·2 aperture, had had two peculiarly fine views of the Sun during the month of March. On the first occasion, during an extraordinary fit of definition, he was tempted to think he had got hold, not exactly of the willow-leaves, but of the rice-grains; but, on looking again, and exchanging a power of 74 for one of 154, he then saw as nearly as possible what Mr. Huggins has described. What he took with a lower power to be an interlacing of these spots resolved itself into a series of dots, separated by darker intervals.

Mr. Brayley remarked that he had received a letter from Mr. Nasmyth, in which he gives four different forms of objects

as he observed them. First, he draws the willow-leaves, No. 1; No. 2 is shorter and a little wider; No. 3 is shorter still and a little wider still; and the fourth is exactly of the rice-grain pattern. He says of No. 1, you have the type of those forming the details of the penumbral strata. No. 2 (a little shorter) is that which forms the details of bridges. No. 3, which is shorter and wider still, is the form which constitutes the other parts of the bridges in the margin of the photosphere. Both 3 and 4 may be said to be a type of those that may be seen over the entire surface of the photosphere.

Mr. Stone stated that he had never seen any quite so long as Mr. Nasmyth's longest. He saw them with about 50, and tried 100 and 150. The higher the power used, the more distinctly were they seen.

Mr. Hodgson thought the discrepancies in the observations might be accounted for by supposing a want of care in obtaining the correct focus—an explanation which the President did not consider sufficient.

At the same Meeting, Mr. Howlett referred to a comparison which he had made between the Ely and Kew photographs of the Sun, taken between the 24th of January and the 1st of February, and exhibited the marked difference in the pictures produced at the same hour, and within a few minutes, at the two different stations. It would seem that the effects of difference of exposure will require to be very carefully considered before we come to any positive conclusion as to what was or was not the state of the Sun's disk at any given hour of the day. Without mentioning more minute differences, there is on the 28th of January a most striking difference between the pictures produced respectively at Kew and Ely, at 11.55 A.M. and 11.23 A.M. respectively. The Ely photographs generally show more of the faint details of the solar disk than those at Kew, but in this instance there is also most certainly a falling-off in that general accuracy and trustworthiness.

On the 28th of January, at 11.15 A.M., there was a very splendid group, which had been for some days previously, and remained subsequently, on the Sun's disk. There was specially to be noted a very large but feebly luminous bridge, spanning about three-fourths of the way across the great central *umbra*. That bridge is about one-fourth of the area of the umbra. There are no visible traces of this in the Ely photograph; while, at the same time, the subordinate spots of the group in the Ely photograph are beautifully and distinctly rendered.

It becomes those well versed in the subject to explain and give us their advice and counsel as to how these discrepancies are to be avoided, or to be interpreted, or perhaps even to be encouraged.

Professor Selwyn remarked: The small exposure of the Ely pictures, whilst it brings out in greater clearness most of the fainter features of the Sun, it fails in those parts where longer exposure seems necessary. He also mentioned that, at the next Meeting of the Society, the whole series of pictures taken during 1863 at Ely (then at the Royal Society) would be exhibited.

The President explained that he pointed out in the Bakerian Lecture for 1861 the cause of these discrepancies. The object of the pictures taken at Kew is not so much the definition of the physical features of the Sun as the obtaining the positions of them with a view to their reduction. There is thus a necessity for a little longer exposure, in order to get the limb of the Sun well brought out; and, in that way, many minor details, such as the details of the penumbra, are lost by over-exposure, which is not required for the Ely photographs. He attributes the delineation of some of the minute work in the Kew instrument to its being really in better chemical focus, evidenced by the details of the faculæ.

The President next adverted to the desirability of obtaining a photographic record, on a large scale, of the physical features of the solar surface, and stated that there was plenty of scope for any observer who should choose to take up the subject, and a very rich reward to be reaped. He had, as far as his leisure permitted, followed up the subject; and now that Professor Selwyn had taken a step in that direction, he would recommend his making the pictures on a larger scale than he at present contemplated — namely, six inches — which was not large enough to show the details of the spots. The image should be projected still further, and the single spots taken very much more enlarged.

Note respecting the List of Double Stars.

By the Rev. W. R. Dawes.

In the remarks appended to the List of New Double Stars, which I recently communicated to the Royal Astronomical Society, I have said, with respect to No. 7 of the List (*Procyon* and its very minute companion), — "I have heard that this minute object has been discovered independently at Mr. Barclay's Observatory; but I am not aware of the date."

I received my information respecting this discovery a year or two ago from Admiral Smyth, who had heard of it from

Mr. Hind; and the date was not mentioned. This evening, while looking over the *Monthly Notices* of last year, I found a communication from Mr. J. Gurney Barclay in the *Notices* for April 1863, respecting this very object; and appended to it are some measures by Mr. Hermann Romberg. I had totally forgotten having ever seen these. The date of the discovery given by Mr. Barclay is "1856, Jan. 10." He describes it as preceding *Procyon* in R.A. by from 3 to 4 seconds of time, "and but litle removed to the North in December." *So* it is printed; but the last word ought, I presume, to be "*declination.*" Mr. Romberg's measurements give about Pos. = 295°, Dist. = 46″.

Hopefield Observatory, Haddenham,
Thame, April 4, 1864.

On the Companion of Sirius. By the Rev. W. R. Dawes.

On two recent occasions I have obtained distinct views of Alvan Clark's close companion of *Sirius* with my 8¼-inch object-glass.

On March 24th the air was rather thick, but the image of the star was unusually quiet and well defined; and though the companion was faint, yet there could be little doubt of its being really a star. To place this beyond suspicion, I turned the object-glass round into various positions, by unscrewing its cell, without any variation in the place of the small star, and also turned the eye-piece, and altered the situation of the object in the field, with the same result. Having applied the filar micrometer, and admitted a slight illumination of the field, I obtained a measure of position with the parallel thick wires = 84°·86; but the small star would not bear sufficient illumination to render the fine webs visible. To obtain a good estimation of the distance, I placed the *thick* moveable webs (which are parallel to the fine webs) at a distance of 15″, and bisected *Sirius* with one of them. The distance of the companion from the centre of *Sirius* was thus estimated to be two-thirds of the distance of the webs from each other, viz. about 10″.

On the 25th I obtained a much better view of this difficult object. The air was clearer, and *Sirius* as well defined as on the previous evening. The companion was so well seen that I had no doubt of getting good measures of distance. My solar eye-piece was on the telescope; and while exchanging this for the micrometer, and applying the illuminating lamp, clouds began to form in that neighbourhood, which soon obscured *Sirius*

itself. The view I obtained, however, fully confirmed the reality of the previous night's observation, and convinced me that good measures might be obtained in a suitable state of the air.

Hopefield Observatory, Haddenham, Bucks,
April 7, 1864.

On the Companion of Sirius. By W. Lassell, Esq.

The recently discovered companion of *Sirius* being an object of special interest just now, I send you a few measures of its position and distance, recently obtained by Mr. Marth and myself, respectively indicated by the letters M. and L.:—

1864·076	M. position 78°·53	mean of	6	measures,	greatest	difference	0°·77
	M. distance 10″·50	,,	6	,,	,,	,,	1″·15
1864·147	M. position 79°·58	,,	6	,,	,,	,,	1°·82
	M. distance 10″·90	,,	6	,,	,,	,,	0″·81
	L. distance 9″·53	,,	6	,,	,,	,,	1″·88
1864·150	M. position 80°·29	,,	6	,,	,,	,,	2°·10
1864·196	M. position 79°·23	,,	6	,,	,,	,,	1°·42
	M. distance 10″·40	,,	6	,,	,,	,,	0″·23
	L. distance 9″·71	,,	9	,,	,,	,,	1″·35
1864·207	L. position 80°·15	,,	6	,,	,,	,,	2°·12
	L. distance 9″·67	,,	6	,,	,,	,,	1″·54

The large difference of measures of distance of M. and L. remains for future explanation. The powers used were 405 and 678, with which, in all tolerable conditions of the atmosphere, the small star could not be overlooked. The difficulty of distance-measures of stars so excessively unequal is, however, extreme, especially in the strong winds which so frequently prevail here in the winter. No other star was detected within the distance of that which has a position-angle of about 165° and a distance of about 2′. The small star appeared to me not a very small point, but to be deficient in brilliancy; it reminded me of the fifth star in the trapezium of *θ Orionis*, which has a dull reddish hue.

The late winter has been, I believe, unprecedentedly unfavourable in this island for astronomical observation; and many objects of interest, such as planetary and other nebulæ, have passed away without opportunity for careful scrutiny. Even for the nebula of *Orion* I must look forward to another

winter to enable me to complete its examination. I have, however, verified several of the very minute stars in the neighbourhood of the trapezium which I discovered in my former expedition (see *Memoirs, Roy. Ast. Soc.*, vol. xxiii. plate iii.) during the remarkably fine winter of 1852–3; and I have detected a few others, though I have not had opportunity in good circumstances to make any very diligent search.

When not engaged with other objects, Mr. Marth has generally used up whatever clear sky we have had, in the absence of the Moon, in searching for new nebulæ, of which there are already near 200 on our list.

Malta, March 26, 1864.

Letter from Herr Theodor Oppelzer to the Astronomer Royal, received April 11, 1864.

(*Translated by W. T. Lynn, Esq. B.A., Univ. London, of the Royal Observatory, Greenwich.*)

"I have the honour of sending you the elements of the Planet (76) *Freia* = (80) *Sappho*. They are, indeed, not yet perfectly accurate, as it was necessary first to wait for Pogson's observations. Nevertheless they already agree tolerably nearly with the observations. In deducing them I have made use of the observations: Copenhagen, 1862, Oct. 24, Nov. 1, Nov. 20, and Berlin, Nov. 27; also Pogson's approximate position, which I assumed to apply to Feb. 10, and my own observation as follows: —

Mean Time Josephstadt.	App. α.	Log (P × Δ).	App. δ.	Log (P × Δ).
1864. h m s	h m s		° ′ ″	
April 2 9 48 12	8 26 38·61	+9·323	+16 48 23·9	(0·674)

"The Elements are —

Epoch = 24·5 Oct. 1862, Mean Berlin Time.

$$M = 312^\circ\ 16'\ 20''\cdot3$$

$$\left.\begin{array}{ll} \pi = & 92\ 52\ \ 4\cdot3 \\ \Omega = & 213\ \ 3\ 21\cdot2 \\ i = & 2\ \ 1\ 51\cdot5 \end{array}\right\}\text{Mean Equinox 1864.}$$

$$\varphi = 10\ 47\ 17\cdot2$$

$$\mu = 569''\cdot3699$$

$$\log a = 0\cdot529742$$

and the places are thus represented, according to a hasty calculation, in the direction Calculation — Observation:—

		δλ.	δβ.
		′	′
1862,	Oct. 24	0·1	0·0
	Nov. 1	+0·1	0·0
	20	+0·2	−0·1
	27	0·0	+0·1
1864,	Feb. 10	+0·3	−0·5
	April 2	0·0	0·0

"In my opinion the identity of d'Arrest's and Pogson's Planets can no longer be doubted. The following is a small Ephemeris, which will, perhaps, be of use. (The planet is of the 12th magnitude):—

0h Berlin Time.

	α	δ	log. Δ
	h m s	° ′	
April 8	8 29 13	+16 44·4	0·389
9	29 45	43·5	
10	30 18	42·5	
11	30 52	41·4	
12	31 28	40·3	0·399
13	32 5	39·0	
14	32 42	37·6	
15	33 21	36·0	
16	34 0	34·4	0·409
17	34 40*	32·7	
18	35 22	31·0	
19	36 4	29·3	
20	36 47	27·5	0·419
21	37 31	25·6	
22	38 17	23·6	
23	39 4	21·5	
24	8 39 52	+16 19·3	0·429

"It would be a thing that I should desire, if you would have the kindness to communicate something of this to the *Monthly Notices.*"

* In the original, 34m 4s.—W. T. L.

Instrument for Sale.

ALTITUDE and AZIMUTH INSTRUMENT, by Thomas Jones, of 62 Charing Cross: cost about 120*l.* Circles about 13½ in. diameter, divided on silver to 10 min. Object-glass of telescope, 1⅞ in. Length of focus, about 19 ins. Between centres of standards. 7½ ins.; between centres of Ys, 13½ ins. There is a spirit-level attached to the upper horizontal plate, to which the standards are fixed. The instrument may be seen at Pastorelli's, 208 Piccadilly.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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WARREN DE LA RUE, Esq., President, in the Chair.

Capt. Basevi, Royal Bengal Engineers;
Albert Escott, Esq., Royal Hospital Schools, Greenwich;
H. P. Finlayson, Esq., Dover; and
R. S. Newall, Esq., Ferndene, near Gateshead,

were balloted for and duly elected Fellows of the Society.

On the Satellite of Sirius. By M. Otto Struve.

The following observations of the satellite of *Sirius*, discovered by Alvan Clark, have been made with our large refractor:—

	Power.	Distance.	Position.	Notes.
		″	°	
1863, March 16	307	10·30	79·8	Distinctly seen.
18	"	9·99	81·2	Good observation. Satellite estimated 8 mag.
1864, March 6	307	11·22	75·2	Good observation. Satellite hardly 10 mag.
19	"		76·6	Bad images; uncertain.
21	"		74·7	Uncertain.
22	"		74·3	Tolerably good, through clouds.
28	"	10·80	75·6	Good observation. Satellite at least 9 mag.
28	412	10·73	74·3	

Hence we have for the mean of the good observations,

1863·21	ρ = 10″·14	P = 80°·5
1864·22	10·92	74·8,

without any difficulty at all. He thought M. Goldschmidt's observations proved this.

Mr. Hodgson said he had seen it more than once on very fine nights with a 6-inch aperture.

On the Probable Error of a Meridional Transit-Observation, by the "Eye-and-Ear" and Chronographic Methods. By Edwin Dunkin, Esq.

In the *Monthly Notices* (vol. xx. p. 86) a short paper of mine is inserted, in which I have given the results of an investigation which was undertaken for the purpose of ascertaining, from the most trustworthy sources, the difference between the mean probable error of a meridional transit-observation made by the ordinary method of "eye-and-ear," and that of a similar observation made by the galvanic or chronographic method. The fundamental numbers were taken from the transits observed with the transit-circle at the Royal Observatory in the years 1853 and 1857 respectively, as published in the *Greenwich Observations* for these years.

An elaborate and important paper on the same subject by the late Dr. C. F. Pape, was subsequently published in the *Astronomische Nachrichten*, Nos. 1284–1286. This interesting communication gave very similar results to those exhibited in my paper, so far as the two were in common.

Very lately, however, my attention has again been directed to these researches by the questions of some visitors at the Royal Observatory. On one or two occasions I have been asked, "What is the difference between the probable errors of transits of stars of the first and sixth magnitudes?" My impressions, as a transit-observer of more than twenty years' experience, have certainly led me to the conclusion that observations of stars of moderate magnitude only, such as the fifth and sixth, convey at the moment of transit the greatest satisfaction to the observer's mind, whether the observation be made by "eye-and-ear" or recorded on the chronographic register; and further, I have also considered that, for exceptional purposes of observation, when extreme accuracy is required, the few brightest stars should, if possible, always be excluded in the night-time from the observing list, on account of the frequent difficulty of estimating the exact centre of the image of the star, which on most occasions has the appearance of a blurred mass of light. This bad definition is very evident on those nights when, from local and other causes, the atmosphere is in a disturbed condition. Similar impressions to the above have probably been made on the minds of most observers

who have been accustomed to regular meridional transit-observations.

The reader will see, however, by the following inquiry, that these impressions, so far as the resulting accuracy of the observation is concerned, are not strictly confirmed, and that the discordance of a single clock-time from the mean of all the observed clock-times, is in reality nearly equal for stars of all magnitudes as far as the sixth.

In the first place to satisfy my own mind, and secondly because this subject is always interesting to practical astronomers, I determined to extend my former investigation, so as to include a greatly increased number of transits observed by the two methods, and to arrange them into six divisions, according to the magnitudes of the stars. In selecting the necessary transits, I have advisedly rejected those of the largest stars when observed whilst the Sun was above the horizon. The stars of the first magnitude used are *Aldebaran*, *Rigel*, *α Orionis*, *Sirius*, *Procyon*, *Regulus*, *Spica*, *Arcturus*, and *α Aquilæ*. Those of smaller magnitude are too numerous for special mention: they have been chosen without partiality for any particular stars, excepting that, to save some computation, those have been preferred which had been the most frequently observed. All the stars are included within 30° of the equator.

The process of reduction may be thus explained briefly:—1. The "eye-and-ear" transits have been extracted from the *Greenwich Observations* for 1853, Section "Transits Observed." To the clock-time recorded at each wire, and which is to be found in the printed volume of *Observations*, a correction has been applied corresponding to the value of the interval between that wire and the mean of the wires. These corrections for the intervals have been specially computed for each star, from the equatoreal intervals published in the Introduction to the *Greenwich Observations*. By the application of the computed intervals to the observed clock-times, I have obtained for each transit seven computed clock-times, all being referred to the mean of the system of wires. A mean of the computed clock-times has then been taken, and the difference of each from the mean found. These differences, or discordances, have then been treated as in my former paper by the formulæ explained in *Calcul des Probabilités et Théorie des Erreurs*, by M. Liagre, Sections 105 to 113. 2. The chronographic transits have been extracted from the *Greenwich Observations* for 1857, except those observed by Mr. Henry, which were taken from the volumes for 1854 and 1855. Mr. Henry's chronographic observations, however, are not included in the following tables, except in that exhibiting the probable errors of the transits of different observers. The method of treatment of the chronographic transits over the nine wires is similar to that adopted for the "eye-and-ear" transits over the seven wires. The

number of transits under discussion was limited to 100 for each magnitude, making, including both methods, a total of 1200 transits.

The following table contains the mean probable error of a transit over *one wire*, and also of a *complete transit*, observed according to both methods. The separate errors have been classified into six divisions, as previously explained; the numbers inserted in the table being the mean of the errors deduced from the observations of each group.

Magnitude of Stars.	Probable Error of a Transit over *One Wire*.		Probable Error of a *Complete* Transit.		Number of Transits over *One Wire*.		Number of Complete Transits by each Method.
	Eye-and-Ear.	Chronographic.	Eye-and-Ear.	Chronographic.	Eye-and-Ear.	Chronographic.	
	s	s	s	s			
1	±0·074	±0·053	±0·028	±0·018	700	900	100
2	0·079	0·050	0·030	0·017	,,	,,	,,
3	0·076	0·052	0·029	0·017	,,	,,	,,
4	0·082	0·052	0·031	0·017	,,	,,	,,
5	0·078	0·049	0·030	0·016	,,	,,	,,
6	0·078	0·051	0·029	0·017	,,	,,	,,
Mean Probable Error, and Total Number of Obs.	±0·078	±0·051	±0·029	±0·017	4200	5400	600

By arranging the separate numbers from which the mean values in the second and third columns of the preceding table are derived, into a series of new groups or divisions according to the N.P.D. of the stars, but without regard to their magnitude, we have the following resulting probable errors:—

N.P.D.	Probable Error of a Transit over *One Wire*.		Number of Observations at *One Wire*.	
	Eye-and-Ear.	Chronographic.	Eye-and-Ear.	Chronographic.
	s	s		
60° to 70°	±0·089	±0·052	651	828
70 to 80	0·082	0·052	1358	1512
80 to 90	0·073	0·051	1169	1512
90 to 100	0·070	0·051	679	558
100+	0·070	0·050	343	990
Mean Probable Error, and Total Number of Observations ...	±0·078	±0·051	4200	5400

A sensible increase is shown in the probable errors of "eye-and-ear" transits as the N.P.D. decreases, as might be

expected. The chronographic transits also exhibit a slight tendency to a similar increase, but its amount is scarcely sensible.

The values given above have been determined from the separate probable errors of transits observed by a considerable number of observers — the majority, however, by the established assistants of the Royal Observatory. It is important and interesting, therefore, again to analyse the separate transits, and to find the mean discordance due to each of the principal observers. The discordances of the computed clock-times, found as previously explained, are not necessarily of larger amount in the transit of any observer whose general method of observing may differ considerably from that of the remaining observers; for it is proved by the following comparison-tables that, though an observer's ordinary personal equation may be of a very different value from that of another, yet the general accuracy of the transit-estimations is equally good in both cases. In skilled transit-observers the ordinary personal equation is usually so constant from year to year, as to make us infer that the same method of observation is continued by the same observer for a long period. There are, however, occasional exceptions to this steadiness of observing, which can be illustrated by noticing the change in the personal equation between Mr. Henry's and Mr. Rogerson's "eye-and-ear" transits, which gradually varied, in the years 1846 to 1853, from $0^s{\cdot}31$ to $0^s{\cdot}67$. The annual change of this value was very remarkable, and was proved to depend in a great measure on a corresponding annual alteration in Mr. Rogerson's method of observing.

To exhibit the yearly uniformity in the methods of observation of the chronographic transits, I have only to extract the "personal equations" of the present principal observers at the Royal Observatory, from the *Greenwich Observations* of the last three years. The observers are, Mr. Stone, Mr. W. Ellis, Mr. Criswick, Mr. Carpenter, and myself. The numbers have been referred, as is usual at Greenwich, to my method of observing, and have been determined from a comparison of the transits of clock-stars observed during the respective years.

	1861.	1862.	1863.
	s	s	s
Dunkin—Stone	−0·13	−0·14	−0·14
Dunkin—Ellis	−0·13	−0·15	−0·16
Dunkin—Criswick	+0·15	+0·15	+0·16
Dunkin—Carpenter	+0·09	+0·08	+0·07

To determine the probable personal discordance of a transit over one wire, I have classified the separate errors into groups according to each of the principal observers. The few occasional transits made principally by the supernumerary com-

puters are placed together in one miscellaneous group. In addition to these, I was desirous to obtain the necessary materials, so as to include Mr. Henry's chronographic observations in this comparison; but in consequence of the death of that gentleman in 1856, I was obliged to extract his transits from the volumes for 1854 and 1855. These transits are therefore supplementary to those which form the basis of the preceding tables. The results are given in the following tables for the six divisions relative to the stars' magnitudes. The mean value for each observer has been found by giving to each divisional result a weight proportional to the number of transits observed over one wire in each division, the number of observations being always considerable.

"*Eye-and-Ear*" *Observations.*

Stars' Magnitude.	Probable Error of a Transit over *One Wire*. Henry.	Rogerson.	Dunkin.	Henderson.	W. Ellis.	Various Observers.
	s	s	s	s	s	s
1	±0·103	±0·062	±0·058	±0·081	±0·074	±0·082
2	0·121	0·051	0·069	0·077	0·071	0·083
3	0·103	0·048	0·062	0·063	0·071	0·096
4	0·109	0·094	0·059	0·076	0·078	0·093
5	0·114	0·056	0·062	0·073	0·061	0·094
6	0·109	0·084	0·061	0·072	0·059	0·086
Mean Probable Error	±0·112	±0·065	±0·062	±0·074	±0·069	±0·089

Chronographic Observations.

Stars' Magnitude.	Probable Error of a Transit over *One Wire*. Henry.	Dunkin.	W. Ellis.	Criswick.	Lynn.	Various Observers.
	s	s	s	s	s	s
1	±0·059	±0·049	±0·058	±0·048	±0·054	±0·056
2	0·059	0·046	0·051	0·044	0·054	0·055
3	0·058	0·047	0·057	0·049	0·052	0·055
4	0·059	0·053	0·053	0·047	0·053	0·059
5	0·054	0·047	0·051	0·046	0·050	0·057
6	0·055	0·049	0·049	0·052	0·048	0·072
Mean Probable Error	±0·058	±0·048	±0·053	±0·048	±0·052	±0·060

A comparison of the two preceding tables exhibits mo[illegible] clearly than any other the great advantages which astronom[illegible] will derive from the substitution of the chronographic meth[illegible]

of recording transits at the Royal Observatory, in the place of the original method of estimation by "eye-and-ear." The great steadiness of the various probable errors in the chronographic transits is something remarkable. This great regularity of transit-observing is very evident, not only from an examination of the values given for the different star-groups of the same observer, but also by comparing those resulting from the observations of the various observers. There is no need, however, to offer any further remarks, as the tables sufficiently explain themselves. But it is interesting to notice that, by the introduction of the chronographic system of registration, the probable error of Mr. Henry's transits was reduced to one-half the amount of that resulting from the original or "eye-and-ear" system of observation.

The numbers which I have hitherto discussed in this paper refer solely to the errors of the actual transit. In chronographic observations these numbers may be composed of three different kinds of error: first, the personal discordance due to a hasty or tardy galvanic contact made at the moment of the star passing the wires in the field of view; secondly, the error of transcribing from the barrel-sheet of the chronograph; and thirdly, the error due to an erroneous value of the intervals between the wires used in these reductions. Of these three sources of error, the two first are, without doubt, the causes of the discordances between the results for each wire and the mean of wires. With respect to the third possible source of error, which is common to both methods, the intervals of the wires are always determined with so much care that it may be safely assumed that their values, so far as this investigation is concerned, are sensibly correct for both systems of wires. In the "eye-and-ear" transits the discordances must therefore rest entirely on the estimations of the observer.

The subject would be incomplete were I not to include in this investigation the determination of the probable error of a Greenwich result for Right Ascension. This has been performed by extracting the separate results, as recorded in the "Ledger," where the mean Right Ascensions are given for January 1. In this instance the probable errors are a combination of not only the errors described above, but also those of any erroneous application of the collimation, level, and azimuth errors, clock-errors, &c. But as these special sources of error must be supposed to affect in a similar manner the mass of the observations by the two methods, the *difference* between the probable errors of the results for Right Ascension deduced from the "eye-and-ear" and chronographic transits should agree with the *difference* between the corresponding errors of a complete transit given in a preceding table, which will be seen is sensibly the case.

For my present purpose I have simply re-arranged the original numbers, the mean results whereof are exhibited in

the table on page 88 of the *Monthly Notices*, vol. xx., into six divisions. By this means I have obtained the probable error of a result for Right Ascension for different classes of stars, as observed at the Royal Observatory in the years 1853 and 1857.

Stars' Magnitude.	Probable Error of a Result for Right Ascension.		Number of Observations.	
	Eye-and-Ear.	Chronographic.	Eye-and-Ear.	Chronographic.
	s	s		
1	±0·059	±0·040	151	168
2	0·046	0·035	205	139
3	0·048	0·033	373	247
4	0·046	0·033	258	256
5	0·052	0·033	132	148
6	0·044	0·035	140	234
Mean P.E. and Total Number of Obs....	±0·048	±0·034	1259	1192

In this table the probable errors of the first group appear to be slightly in excess in both methods. I think this may be accidental, and arises probably from some of the other sources of error, rather than from the stars' magnitude.

In laying before the Society the preceding abstracts, which are the results of a great mass of computation, I consider that I can with some confidence offer the following conclusions, which are derived from this discussion:—

1. In "*eye-and-ear*" observations, the probable error of a Greenwich transit observed in 1853 over *one wire* is $\pm 0^s{\cdot}078$, while that of a *complete* transit over the seven wires is $\pm 0^s{\cdot}029$. In *chronographic* observations, the probable error of a Greenwich transit observed in 1857 over *one wire* is $\pm 0^s{\cdot}051$, and that of a *complete* transit over the nine wires is $\pm 0^s{\cdot}017$.

2. There does not appear to be any certain difference in the probable error of transits of stars between the first and sixth magnitudes.

3. In "*eye-and-ear*" transits, for stars whose N.P.D. is greater than 60°, it would seem that the probable error of a transit increases slightly as the N.P.D. decreases; while in the *chronographic* transits the corresponding changes are insignificant.

4. In "*eye-and-ear*" transits, the personal discordances are liable to a considerable variation between the different observers; in *chronographic* transits, the differences between the observers are comparatively small. The general steadiness of observing by the latter method is very remarkable, and shows the great advantage obtained by its adoption.

5. The probable error of a Greenwich result for Right Ascension in the year 1853, as determined from "*eye-and-ear*"

transits, is $\pm 0^s{\cdot}048$; while the corresponding probable error resulting from *chronographic* transits, in the year 1857, is $\pm 0^s{\cdot}034$. By arranging the separate results into groups, according to the stars' magnitude, it is found that no certain difference can be distinguished in the accuracy of the observed transits, excepting only that there is a tendency in both methods towards an increase in the probable error when transits of stars of the first magnitude are observed. The amount of increase in this instance, however, is in reality small, and is probably to some extent accidental.

22 *Royal Circus Street, Greenwich,*
April 18, 1864.

Mr. De La Rue remarked that it was impossible to listen to papers such as the foregoing one without congratulating ourselves upon the exactness of modern observation, and upon the industry of our lovers of science, both amateur and professional. Looking to the result of the calculations contained in Mr. Dunkin's paper, one was almost overcome by the amount of persevering industry which must have been required for bringing observations to such a point of excellence. He then referred to an improvement upon a proposal, made some time ago by Professor Wheatstone, for increasing the accuracy of transit observations. Most of the members present would be aware that there was a personal equation existing even in the chronographic system. The finger might move a little too soon or too late upon the tappet, and the tappet might hang fire, and a number of other circumstances might occur. This, Professor Wheatstone proposed, should be remedied by a system of wires in the transit instrument, which, when the star was brought between them, should follow its movement; and when the star passed the optical axis of the instrument, or any number of known points from that axis, then the chronographic contact would be made by the wires making contact, so that a number of records would be obtained independent of the will of the observer, and only dependent upon the irregularity of the movement in following the star — a method very similar to that employed by the speaker in his first lunar-photographic experiments.

Col. Strange, in a recent visit to Paris, had consulted the astronomers at the Imperial Observatory as to the value of the chronographic method, and had received a report very decisive against it, a very strong opinion having been expressed that it did not exceed the old method in accuracy, even if it came up to it. M. Le Verrier's remark in addition being, "*Cela fait de mauvais astronomes.*" He was however still of the opinion

that the Greenwich method was the more sound of the two. He had therefore, with the sanction of the Government, ordered a complete chronographic apparatus for the use of the Indian Survey. He believed that M. Le Verrier's condemnation of this process must have been based upon a comparatively small number of observations; whereas, where we have to deal with the fractional part of a second, a few observations were of very little value. The French astronomers performed their experiments no doubt with their usual skill, but they had not the advantage of an accumulation of data so extensive as that which the Society had just had laid before them. With reference to what had fallen from the President, Col. Strange remarked that he believed the method proposed by him of having a telescope so constructed that the star should automatically be followed over the wires had been already achieved: he had examined, while in Paris, an apparatus of this description, invented by M. Redier, in action, at the Paris Observatory; the wire was carried so steadily across the field — so exactly with the same velocity as the passage of the star — that the intersection of that star with the wire was a matter of the most perfect ease and certainty. In fact there was ample time for the observer to call an assistant to verify the observation before leaving the instrument. This system, together with the chronographic method, could not fail to reduce to within even smaller limits, those already excessively minute errors with which Mr. Dunkin had so ably dealt in his paper.

Mr. Walker said he had long been convinced that electricity would render very great service to astronomy, and his predictions had been verified by what they had just heard from Mr. Dunkin.

Mr. Dunkin, in reply to Colonel Strange's allusion to the objection urged by some astronomers to the chronographic system, that it tended to produce bad eye-and-ear observers, remarked, that so far from that being the case, it was found in practice that the old observers at the Royal Observatory, who had not practised eye-and-ear observations for ten years, when the clock was out of order, and they had recourse to the old method, made their observations just as accurately as they did ten years ago.

Mr. Pritchard said that perhaps few people had an idea how very small the present errors in observations really were. The whole extent of them was simply this: the third of a second of an arc might be represented by putting a human hair at the distance of 125 feet.

The following paper was then read by Mr. Dawes :—

Results of some recent Observations of the Solar Surface, with Remarks. By the Rev. W. R. Dawes.

During nearly the whole of last year and the first three months of this, the state of the air was rarely sufficiently good to permit a successful scrutiny of the solar surface with high powers on a large telescopic aperture;—on such occasions, at least, as I was able to make much use of. But on three or four days in April I found that vision was remarkably steady and distinct, and I improved the opportunities to the utmost.

As it was of considerable importance that the same minute portions of the surface should be examined and compared together under various magnifying powers, and with different shades of dark glass, I preferred, after a careful comparison with the ordinary eye-pieces and an excellent transparent diagonal, to employ a solar eye-piece of my own construction. And I would take this opportunity of correcting an idea which has been advanced, that the diffraction of the rays at the edge of a small aperture in that eye-piece interferes with the distinctness of the object viewed. Close to the edge of the field there is, of course, as Mr. Huggins well expressed it at the last Meeting of the Society, a narrow ring of diffracted light: but the breadth of this ring does not, I think, exceed 2″ on my telescope; and the smallest aperture in the diaphragm-plate has a diameter of 15″. Consequently there is a central space of at least two-thirds of the diameter in which the definition is perfect, even in the smallest aperture of the eye-piece. Yet, for the purpose now referred to, I rarely use a field of much less than *one minute* in diameter; and any object not exceeding 45″, when placed in the centre of the field and kept there by the driving-clock, is recognised with certainty under different powers, and seen with the utmost possible distinctness.

1. One of the objects I had principally in view was, to ascertain whether in any part of the photosphere any objects could be found which could reasonably be compared to willow-leaves in their form.—It may be well to state here that I have always found it difficult to devise any appropriate appellation for the small bright irregularities of the surface, which would avoid an assumption of their character, or ascribe to them a regularity of form they do not possess. In my first paper I expressed my strong objection to any *name* of this kind, as calculated to convey an erroneous impression. The term *willow-leaves* seemed utterly inapplicable to anything I had ever succeeded in discovering. A far less objectionable term, as it appears to me, is that of *rice-grains* applied by Mr. Stone to those objects with which all careful Sun-observers must be acquainted, as there is no difficulty in seeing them in a moderately favourable state of the air, and which have been familiar to myself for many years;—so much so, indeed, that

when they were not discernible I invariably abstained from any further scrutiny of the solar surface as being useless. Yet even this appellation conveys the idea of uniformity of shape and size which these objects do not possess, and is, I think, on that account objectionable. But I have been led by it to apply the term *granulations*, or *granules*, which assumes nothing either as to exact form or precise character; and I venture to hope that the term will be generally adopted.

To proceed then with the results of my recent observations.

Various portions of the surface were diligently examined with powers from 131 to 407. The bright little granulations were easily seen in all parts not very near the edge of the disk. Their forms and sizes were carefully noted, and found to be so various as to defy every attempt to describe them by any one appellation or comparison. But, as I have observed in a former communication, the rarest of all forms was the long and narrow; and in no instance did I succeed in finding one which could with propriety be compared to a willow-leaf. Occasionally, some that were nearly in contact differed so greatly in size that one was four or five times as large as the other: and while, in a remarkably bright mass, one somewhat resembled a blunt and ill-shaped arrow-head, another, very much smaller and within 5″ of it, was an irregular trapezium with rounded corners. The more quiet and perfect the views I obtained of them, the more convincing did the evidence appear that they were not individual and separate bodies of a peculiar nature, but only different conditions as to brightness or elevation of the larger masses forming the mottled surface; just as the brighter portions of that surface, and the *faculæ* also, are different conditions of the general photosphere. In these researches I met with nothing which had the slightest resemblance to the *interlacing* which Mr. Nasymth has so clearly described and so distinctly depicted in his communication to the Manchester Literary and Philosophical Society; of which letter I was not aware when I penned my former paper on this subject. Mr. Nasmyth having subsequently favoured me with a copy, I was so struck with the clearness and decision of his assertions that I began to think I must have overlooked the peculiar appearances of the objects which he has depicted in his diagram as being "*the exact form of these remarkable structural details*," which he describes as "*forming the entire luminous surface of the Sun;*" and, therefore, to leave nothing untried, I collected all the information I could as to the means employed by him and by other observers who had seen something of the same kind, in the hope that some change in my apparatus or mode of using it might at length render me successful. Now, however, it appears that Mr. Nasmyth has withdrawn his former statements as to the exact and uniform figure of the objects he claimed to have discovered on the entire surface; and that, in fact, all that might have

been regarded as a *discovery* resolves itself into an appearance perfectly well known many years before.

The darker or shaded lines between the granules I distinctly observed in many places to be pretty thickly covered with dark dots, like *stippling with a soft lead-pencil;* and these are what have been called "*pores*" by Sir John Herschel, and "*punctulations*" by his father. Some of these were almost black, and looked like excessively small spots just breaking out. But none of them were seen to enlarge or materially alter their form, though at times so sharply defined with powers 276 and 407 that it was obvious they were in general not quite round.

It appears to me quite incomprehensible that, under such definition, any bright objects of peculiar form should have escaped detection when specially looked for; and I was certainly struck with the extreme rarity of a long and narrow shape among the hundreds of granules which I examined in those four days' observations. If therefore there are any willow-leaf-shaped objects to be seen, they must be quite distinct from these granulations, and can form no regular or usual ingredients in the composition of the solar surface.

On the *penumbræ* of the spots then visible I found several long and narrow bright lines, extending like bits of white thread completely across the whole breadth of the penumbra without any break whatever; and, though there were several smaller pieces, not one of them would have suggested to my mind the comparison with a willow-leaf. Perhaps the bright granules of the general surface may sometimes be compressed into a longer form under the influence of the same forces which produce the longer threads or *straws* on the penumbra; but one of the most striking features of that part, as well as of the general surface, is, according to my observations, the entire absence of uniformity in the brighter portions with respect both to their size and shape.

2. A second object of my investigations was to determine whether the granulations existed equally in the brightest and in the less luminous portions of the surface, which together form the general and comparatively *coarse* mottling of the photosphere.

On comparing together in the same field of view several of the more and less luminous masses which produce the coarse mottling of the surface, I came to the conclusion that the granules are generally *larger* as well as brighter on the brightest parts than on the darker; the difference in brightness of the individual granules in each part being much the same as in the different masses themselves. One fact struck me as very remarkable, that, on each of these larger masses, the individual granules are all very nearly of *equal* brilliance throughout the mass to which they belong. They are not in general, if ever, mixed together, some much brighter and

others far less bright on the same mass. Occasionally I have noticed that some of the brighter masses are decidedly brighter than others of the same class, and that such have extended to a much greater length than usual. The granulations on them are also generally, I think, of uncommon size, and the shaded lines between them of a lighter tint. I strongly suspect that these specimens, if near the Sun's edge, would be seen as *faculæ*.

3. A third principal object I had in view was, to scrutinise especially the brighter parts immediately surrounding the penumbræ of spots (which in a former paper I have referred to as indicating a heaping up of the photosphere by some eruptive force proceeding from the centre of the spot), with the purpose of ascertaining whether the granulations elsewhere easily visible were to be found there; and also to examine the *faculæ* of various forms, sizes, and degrees of brightness, to determine whether any such granulations were visible upon them.

With reference to the reality of this brighter region surrounding all the considerable *profound* spots, and also some of the smaller ones, I have lately been much gratified by the corroboration of the fact afforded by some solar photographs taken by our President, and obligingly presented to me since the publication of my former paper. While the general mottling of the surface is well brought out in them, they show that this mottling never extends to the margins of the spots; which precisely agrees with my observations. In this locality I have never been able to detect any of the granulations so abundant and so easily seen elsewhere. My recent observations fully confirm the conclusion I have formerly arrived at, namely, that the commotion, of whatever kind it might be, had produced the effect of heaping up the material of the photosphere; and had confounded all the distinctions elsewhere seen of the large brighter and shaded masses, and also of the granulations on both, and the pencil-lines, occasionally stippled, by which the individual granules are distinguished.

Precisely the same results attended my examinations of the *faculæ*. Nothing resembling granulations could be found on any of them, whether they were the tortuous, thread-like objects seen near the east and west borders, or the shorter lumps of apparently similar composition near the poles. Yet the granulations were discernible in several places nearer to the Sun's edge than the faculæ were situated; so that it could not arise from the obliquity of the view that they were not visible on the faculæ themselves. I conclude therefore, that the same disturbance which produces the elevated ridges confuses the minute features elsewhere seen; and that, though there may be some traces of granulation when the faculæ are viewed almost perpendicularly, yet this is entirely lost when their sides only are seen near the Sun's limb. In this position,

however, there are often distinct evidences of irregularity in the elevation of different parts of the ridge; and these may, perhaps, when viewed perpendicularly, produce variations of brightness, like the granules of extraordinary size mentioned above.

Hopefield Observatory, Haddenham, Bucks,
1864, May 9.

On the Appearances of the Sun's Disk.
By C. G. Talmage, Esq.

I should have ventured to communicate the contents of this paper to the Society at an earlier period, had I not felt some diffidence in saying that I have never been able to see those objects which are so positively declared to exist by persons who may be considered authorities on such matters; but when it is stated by a gentleman who possesses the great experience and acute vision of the Rev. W. R. Dawes, I feel a little more confidence in giving the result of my own observations.

Since 1861, November, I have most carefully searched for Mr. Nasmyth's "willow-leaves," under various circumstances.

From 1861, November, to 1862, April, I was engaged in astronomical pursuits at Nice, and on every fine day I examined the Sun with one of three Equatoreals—a 4-inch, a 6-inch, and an 8-inch (belonging to Mr. Coventry)—with powers from 40 to 500. In the summer of 1862 I examined the Sun at Paris with another 8-inch object-glass of exquisite definition. In November of the same year I revisited Nice; and until my return, in 1863, April, I omitted no opportunity of searching for the "willow-leaves," but never have I seen the slightest trace of "willow-leaves," "rice-grains," or "thatch."

During this spring I have also examined the Sun with a small telescope, fitted with a binocular eye-piece belonging to a first-class microscope, and without any greater success, though the spots are clearly shown as funnel-shaped depressions or holes: all that I see on the Sun is merely a difference of greater or less luminosity.

A remark was made at our last Meeting that perhaps these appearances depend on focus, and the examination of diatomaceæ out of focus was given as an instance. When I returned home I examined several specimens of diatomaceæ, both easily resolvable, and tests (*Navicula cuspidata*), with various powers from 3-inch to a $\frac{1}{8}$-inch, in and out of focus; and I could compare them to nothing but diatoms out of focus; there was not

the least resemblance to anything I have ever seen on the solar disk.

Mr. Bishop's Observatory, Twickenham,
April 18, 1864.

Mr. De la Rue, referring to the full discussion which the subject of Mr. Dawes' communication had received at previous meetings, remarked that it is to the Sun himself, and to other observers, that the confirmation or non-confirmation of Mr. Nasmyth's discovery must be left. Mr. De La Rue maintains, notwithstanding what had fallen from Mr. Dawes, that it was a substantial discovery. Mr. De La Rue had recently fitted up one of Sir John Herschel's prism-eye-pieces, with a view of continuing these observations upon the Sun, and he has also designed a prism for his Newtonian Reflector, with the same view, in order to collect more facts on this subject.

Mr. Dawes said he thought one of the most remarkable things connected with the matter was that, whereas these granules, or "rice-grains," were easily seen—a small telescope, with a power of 40 or 50, bringing them into view—Mr. Nasmyth should have accepted them as his "willow-leaves," which, he says, are so difficult to see with an 8-inch aperture.

Mr. De la Rue said that, with regard to one person having observed them with a comparatively small power, he might state he could recollect the time when he could see perfectly lines and spaces of 200 to the inch; he could not do so now without a magnifier, and he thought this apparent contradiction might be accounted for in the same manner.

Mr. Stone said the appearances presented to him were what he had described, viz., luminous particles scattered over the Sun's disk, which, by way of description, he had called "rice-grains." He was not prepared to say that Mr. Dawes' "granules" were not the same things; all he could say was, that he was not aware of his having published any account of them. He felt very great confidence in the existence of what he had described, and he had examined them with magnifying powers up to 400, and had not found the slightest change in their appearance. He was not prepared to say that it might not be a very peculiar appearance of the luminous clouds, such as Mr. Dawes suggested; but he considered that, even supposing it to be so, it was well worth the attention of the astronomer.

Mr. Howlett corroborated a remark made by Mr. Dawes as to the general presence of a bright annular appearance, just outside the penumbra of a spot. He next alluded to the eye-piece recommended by Sir John Herschel for observations of the Sun by means of screen projections in a darkened chamber; the construction of which, it was remarked by Mr. Pritchard, is not at all difficult.

Comparison of the Chinese Record of Solar Eclipses in the Chun Tsew with the Computations of modern Theory. By the Astronomer Royal.

In the *Monthly Notice* for 1863, Dec. 11, our Assistant Secretary, Mr. Williams, has given an abstract of the record of thirty-six eclipses in the Chinese historical work called *Chun Tsew*. Mr. Williams has, with great care, converted the Chinese dates into dates according to the Julian calendar.

The period through which these eclipses extend is included in the time through which calculations of eclipses have been made in the French work entitled *L'Art de vérifier les Dates*. I have several times had occasion to re-calculate with great accuracy eclipses which are noted in that work (edition 1820); and I have found that, to the limits of accuracy to which it pretends, and which are abundantly sufficient for the present purpose, it is perfectly trustworthy. I have therefore made a comparison of the *Chun Tsew* eclipses with those of *L'Art de vérifier les Dates*. The result is interesting.

Of the thirty-six eclipses, thirty-two agree with those of the *Art de vérifier les Dates*, not only in the day, but also in the general track of the eclipse as given in the *Art de vérifier*, which appears to show sufficiently that the eclipse would be visible in that province of China to which the *Chun Tsew* is referred.

The eclipses

No. 10, B.C. 645, April 15,

and

No. 15, B.C. 592, June 16,

appear impossible. There cannot have been eclipses in April of B.C. 645, or in June of B.C. 592. It appears, however, from a note with which Mr. Williams has favoured me, that the date attached to No. 10 is in reality an erroneous repetition (in the Chinese mode of expressing it) of that attached to No. 11; and, in the absence of correct date, it must be rejected. In the record of No. 15 no clerical error is found, and there must therefore be error of a different class.

The eclipses

No. 22, B.C. 552, September 19,

and

No. 25, B.C. 549, July 18,

to which there is nothing corresponding in the *Art de vérifier*, are in a different category. These occur in the lunations immediately succeeding No. 21, B.C. 552, August 20, and No. 24, B.C. 549, June 19, respectively; and there is no doubt that Nos. 21 and 24, which agree with the *Art de*

vérifier, were real eclipses. Now there cannot be eclipses visible at the same place in successive lunations; because the difference of the Moon's longitudes is about 29°, and the difference of latitudes is therefore nearly 3°, which is greater than the sum of diameters of the Sun and Moon increased by any possible change of parallax for the same place. Nos. 22 and 25, therefore, were not real eclipses. It seems probable that they have been set down by the observer in his memorandum-book as days on which eclipses were to be looked for. I conjecture that the eclipses Nos. 21 and 24 were observed by one and the same person, and that he possessed science enough to make him connect the solar eclipses with the change of the Moon, but not enough to give him any idea of the limitations to the visibility of an eclipse.

There is no instance, among these eclipses, of recurrence of an eclipse at the end of a Saros; but there are eight instances of recurrence at the end of a triple Saros, namely,

No. 2	to	No. 8,	B.C. 709,	July	17,	to	B.C. 655,	August	19
8	,,	13,	655,	August	19,	,,	601,	Sept.	20
12	,,	19,	612,	April	28,	,,	558,	May	31
16	,,	30,	575,	May	9,	,,	521,	June	10
17	,,	31,	574,	Oct.	22,	,,	520,	Nov.	23
18	,,	34,	559,	Jan.	14,	,,	505,	Feb.	16
21	,,	35,	552,	August	20,	,,	498,	Sept.	22
24	,,	36,	549,	June	19,	,,	495,	July	2[illegible]

Royal Observatory, Greenwich,
April 5, 1864.

Occultations of Stars by the Moon, and an Eclipse of Jupiter's Third Satellite, observed at the Royal Observatory, Greenwich, in the Month of March, 1864.

Occultations of Stars by the Moon.

Day of Observation. 1861.	Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
March 18	A^1 Cancri, disapp.	Dark	6 58 30·6	J. C.
	A^2 Cancri, disapp.	Dark	7 57 27·8	J. C.
	A^1 Cancri, reapp.	Bright	9 42 54·6	J. C.
	(*a*) A^2 Cancri, reapp.	Bright	11 3 0·8	J. C.
March 19	(*b*) ω Leonis, disapp.	Dark	6 11 26·7	E.
	ω Leonis, reapp.	Bright	7 28 34·0	E.

Eclipse of Jupiter's Third Satellite.

March 11. Disappearance of third satellite $16^h\ 3^m\ 50^s{\cdot}7$ R.

(*a*) Probably not correct to less than a second.

(*b*) Strong daylight and much tremor. The star seemed to diminish in brightness about a second before extinction.

The initials E., J. C., and R., are those of Mr. Ellis, Mr. Carpenter, and Mr. Roberts.

Occultations of Stars by the Moon. Observed by Capt. W. Noble, at Forest Lodge, Maresfield.

Friday, March 18th. *Occultation of* A^2 *Cancri.*

The star disappeared instantaneously

at $9^h\ 30^m\ 37^s{\cdot}2$ L.S.T. = $9^h\ 43^m\ 19^s{\cdot}7$ L.M.T.

Power 115, adjusted on the star.

Saturday, March 19th. *Occultation of* ω *Leonis.*

The star disappeared very nearly instantaneously

at $6^h\ 1^m\ 34^s$ L.S.T. = $6^h\ 11^m\ 24^s{\cdot}9$ L.M.T.

and reappeared

at $7^h\ 19^m \pm 8^s$ = $7^h\ 28^m\ 46^s{\cdot}2$ L.M.T.

There was a good deal of atmospheric disturbance, and the twilight was very strong at the time of disappearance. The star was caught almost at the instant of its emergence from the Moon's bright limb.

Power 154, adjusted on the star. In each case my Ross equatoreal of 4·2 inches aperture was employed.

On the Minor Planet (70) *Freia.* By N. Pogson, Esq.

The following observations, by Mr. Pogson, of an asteroid, originally observed for *Concordia*, have been extracted from a letter to the Astronomer Royal, dated Madras, March 28, 1864:—

1864.	Madras M.T	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
Feb. 2	15 13 28	8 42 56·76	74 59 16·0
9	12 45 41	8 37 39·26	74 36 44·9
10	13 0 1	8 36 54·90	74 33 28·0
16	14 9 6	8 32 46·35	74 14 43·7

Magnitude 11·6.

It would appear from the calculations of Herr Theodor Oppolzer (*Ast. Nach.* No. 1471), that these are really observations of the planet (76) *Freia*.

Notes on α Centauri and other Southern Binaries, and on the Nebula about η Argûs. By E. B. Powell, Esq.

A brief notice of the motion, during the last two years, of *α Centauri*, may be of interest to some of the members of the Society.

In my observations of double stars, taken in 1859–62, and communicated towards the close of the latter year, the last mean measures of *α Centauri* were as follows:—

Epoch.	Position-Angle.	Distance.
1862·205	358°·0	6″·79

I have since secured the means given below:—

Epoch.	Position-Angle.	Distance.
	°	″
1863·028	1·4	7·2
1864·110	5·7	7·85

The Position for 1863 is derived from 59 observations on 6 nights; the Distance, from 36 measures on 4 nights. The separate nights did not furnish as accordant Position-results as was desirable: in fact, the latter varied between 0°·83 and 2°·2. The results for the current year are very accordant, and I have great reliance upon them. The Position is the mean of 64 measures on 7 nights, and the Distance that of 70 measures on 5 nights. The whole range of the measures for the different nights was, for Position, only ½°, and for Distance, 0″·35.

By drawing a Position-curve, it was found that the comes was on the same meridian with the primary about 1862·65; the Distance must then have been very nearly 7″.

I beg to invite attention to the important portion of the orbit now about to be described, viz. the part in the immediate neighbourhood of the lesser maximum of distance. If this maximum be accurately determined, one most prominent feature in the path will be fixed; and then, as the companion will revolve with continually increasing rapidity till its distance from the primary diminishes to 1″ or less, a really excellent orbit will be calculable in 1870, or thereabouts. I may remark that the orbit contained in the notes to my observations of 1859–62, though agreeing tolerably with observation up to this time, assigns too small a magnitude to the lesser maximum distance.

The following are my latest measures of two other Southern binaries:—

Star.	Epoch.	Position.	Distance.
		°	″
p Eridani	1863·017	250·9	4·88
γ Coronæ Aust.	1863·836	318·1	1¼ est.

The latter star is moving rapidly, though not so rapidly as was anticipated; it has described about 20° in the last four years.

In the note on *η Argûs*, in my paper already alluded to, I called attention to two points connected with the Nebula about that star: (1), that the whole Nebula had faded away very considerably in 1860; and (2), that it had altered its form, the nebulous matter having receded so as to leave open the southern end of the lemniscate vacuity. These remarks were based upon numerous observations taken in 1859, 60, 61, and 62; and I will now give a few particulars, which appear to me to possess some interest.

On the 15th April, 1860, I noted in my observation-book that the Nebula was "much fainter than formerly;" while, on the immediately preceding 23d March, the following entry occurs:—"Nebula about *η Argûs* magnificent." On the date first mentioned, *η* is in a rough sketch placed out of the bright portion of the Nebula, and the lemniscate is described as a "channel." Several entries follow, noting both the openness of the lemniscate to the south and the greatly diminished brightness of the Nebula. The former fact was acquiesced in by Major Tennant of the Bengal Engineers, whom I requested to examine the Nebula with the Lerebours Equatoreal of the Madras Observatory. On the 4th April, 1862, the following entry was made in my book:—"*η* beautifully round and clean, out of nebulous matter altogether; two patches of nebulous matter, with a passage between them, to the left or preceding."

The original object with which I turned my attention to the locality of *η Argûs* was to make micrometrical measurements of *η* and the neighbouring stars, for the purpose of comparing

the same with Sir John Herschel's *Cape Observations;* as subsidiary to the above, I made notes of the variations in brightness of η, but the changes in the Nebula were not dreamt of; and, in fact, I was at first inclined to fancy that they were only apparent, and due to the use of a different and inferior instrument.

I have to observe that to Mr. Abbott must be ascribed the first publication of the fact that η is no longer in the dense portion of the Nebula where it was seen by Sir John Herschel.

Madras, March 9th, 1864.

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MONTHLY NOTICES

OF THE

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Dr. LEE, Vice-President, in the Chair.

Samuel Player, Esq., Duxford, Cambridgeshire, was balloted for and duly elected a Fellow of the Society.

On the Transit of Venus, 1882, *December* 6.
By the Astronomer Royal.

The *Monthly Notice* for 1857, May 8, contains a communication made by me to the Society on the means available for correcting the received measure of the Sun's distance from the Earth. Among the different subjects of observation applicable within some years to this object, I have particularly indicated the Transit of *Venus*, 1882, December 6, treated by the method of comparison of durations of transits at different places, as one which merits most careful consideration.

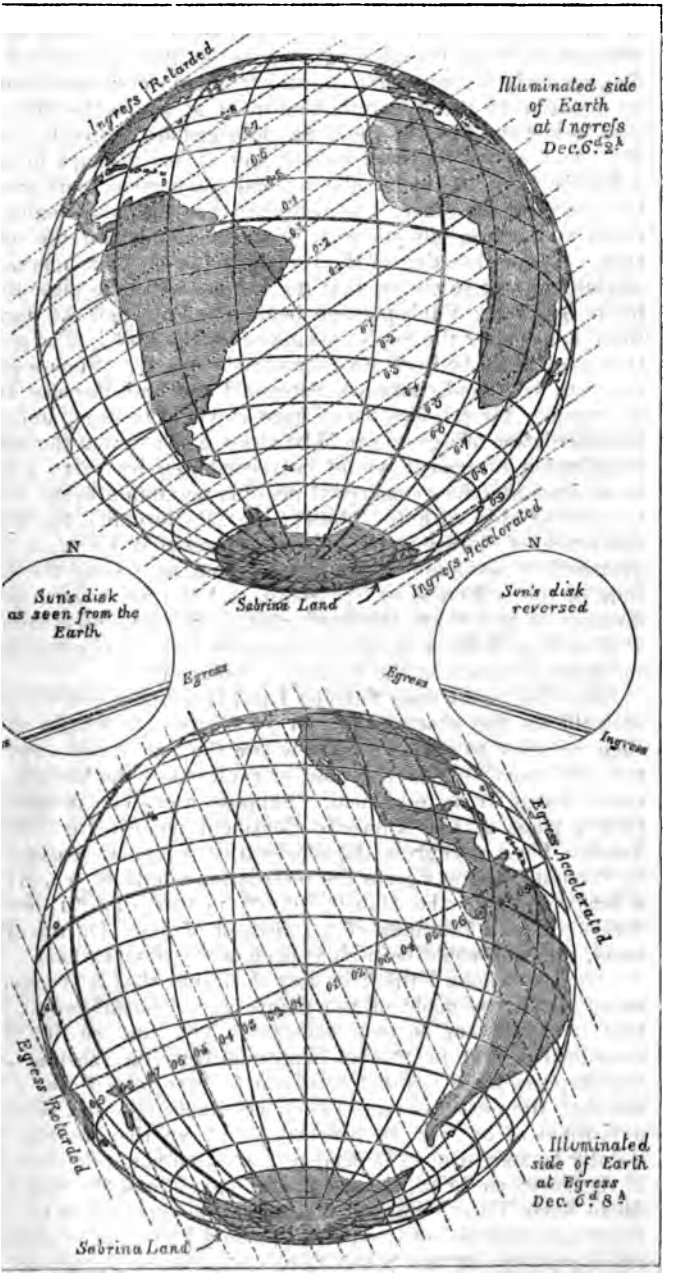
The small Maps of the Illuminated Sides of the Earth at the time of ingress and egress of *Venus* (accompanying my paper) were drawn on a projection which exhibited with sufficient clearness the countries where the transit would be visible, but which did not give facilities for estimating numerically the effect, at each locality, of the relative parallax of *Venus* and the Sun. A moment's consideration will show

that, if at each of the critical phenomena (Ingress and Egress) a plane be drawn cutting the centre of the Earth and touching that part of the Sun's limb at which the phenomenon happens, then the amount of acceleration or retardation of the time of the phenomenon, at any special locality, as attributable to parallax, depends merely on the magnitude of the perpendicular from that locality to the plane just mentioned. If an orthographic projection of the Earth's illuminated face be drawn, the curve upon the Earth's surface, which passes through all the points at which that perpendicular has a determinate value, is represented in that projection as a straight line, parallel to the line which represents the first-mentioned cutting plane. And, therefore, if we wish to indicate those points of the Earth at which the parallactic effect is $\frac{1}{10}$th or $\frac{2}{10}$ths of the maximum, we shall only have to draw, on such a projection, lines parallel to the central line which itself is parallel to the limb at which the phenomenon takes place, at distances from the central line equal to $\frac{1}{10}$ and $\frac{2}{10}$ of the Earth's radius.

At my request, Mr. Carpenter has prepared the accompanying Orthographic Projections of the Illuminated Sides of the Earth for Ingress and Egress, and has placed on them the lines marking the localities at which the effect of parallax will be $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, &c. of the maximum effect in both directions. A small circle for the Sun's disk and lines for the apparent path of *Venus* are also shown; and likewise the same circle and lines reversed (right to left), because in that state they represent the points of Ingress and Egress, as viewed in the same direction in which the illuminated side of the Earth is viewed, and therefore enable us at once to fix on the points of the Earth's limb where the parallactic effect is maximum.

Selecting, then, first the parts of the Earth at which the duration of transit would be shortest, it is seen at once that, in the seaboard of the United States of America, the Ingress is retarded by a quantity represented by 0·95, and the Egress is accelerated by a quantity which, on the mean, is 0·83 nearly; so that the whole shortening is represented by 1·78 (the geometrical possible maximum being 2·00). That locality, therefore, is very favourable. The Southern States, as far as the Gulf of Mexico, without much diminution of parallactic effect, would have the advantage of a higher Sun at Egress. Another favourable place is Bermuda; the parallactic number for Ingress is 0·9, and that for Egress is also 0·9; so that the whole shortening is 1·80. The circumstances, therefore, are exceedingly favourable for the selection of observing-stations at which the duration of transit will be much shortened.

Selecting, secondly, the parts of the Earth at which the duration of transit would be longest, it will be found that the choice is more limited, and the practical difficulties rather greater. For the acceleration of Ingress, at 2^h Greenwich


Ingress Retarded
Illuminated side of Earth at Ingress Dec. 6d 2h
Ingress Accelerated
Sabrina Land
N
Sun's disk as seen from the Earth
Egress
N
Sun's disk reversed
Egress
Ingress
Egress Accelerated
Egress Retarded
Illuminated side of Earth at Egress Dec. 6d 8h
Sabrina Land

mean time, the observing-station ought to be on the right side of the diagram; and for the retardation of Egress, at 8^h Greenwich mean time, it ought to be on the left side of the diagram. It is impossible to satisfy these conditions, except by a station on the Antarctic Continent at which the Sun will pass below the Pole in the time intermediate between 2^h and 8^h. And, as the Sun will be low, and as any attempt to gain a higher Sun at one of the critical phenomena will give a lower Sun at the other, it is desirable that the hour-angles be equal at the two, but one west of the meridian and the other east. As the duration of the transit is (nearly) six hours, the station must be so chosen that the Ingress will take place three hours before the Sun's passage below the Pole, and the Egress three hours after the Sun's passage below the Pole. The place, therefore, must be in 7^h East Longitude nearly. Such a position can be found, between Sabrina Land and Repulse Bay. Its number for acceleration of Ingress would be 0·95, and that for retardation of Egress would be about 0·66; so that the whole lengthening of transit would be represented by 1·61; a very large amount (the geometrical possible maximum being 2·00). Combining this with the observations at Bermuda, the whole difference of durations would be represented by 3·41 (the geometrical maximum being 4·00). And, supposing the latitude of the station to be 65° South, and the Sun's south polar distance to be 67° 27′, the Sun's altitude at each observation is 4° 6′; which, if the weather is favourable and the sky not liable to banks of haze near the horizon, is sufficient.

But this point near Sabrina Land is the only one which is suitable for the observation. If we change the station at all, then, in order to obtain a place where the Sun at one observation will not be excessively low, or even below the horizon, we must change it a great deal. Suppose, now, for example, we took a point of the Antarctic Continent opposite to Sabrina Land. Then at Ingress the acceleration would be represented by 0·5 only, and at Egress the retardation would be 0·1, giving a lengthening of the transit by 0·6 instead of 1·61, and a difference with Bermuda of 2·4 instead of 3·4. On the other hand, the Sun would be high at each of the observations.

On the whole, I think it very desirable that a reconnaissance should be made of the points under consideration, and that it should not be long deferred. The first locality to be examined is that in 7^h east longitude, between Sabrina Land and Repulse Bay; and the points to be ascertained are,—(1), whether the coast is accessible on December 6,—(2), whether a latitude of 65° can be reached,—(3). whether the Sun can usually be seen well on December 6, at three hours on each side of the lower meridian, or more precisely, at 2^h and 8^h Greenwich Mean Solar Time. Should the answer to the first or third of these questions be negative, then it would be proper to examine other portions of the South Continent, say in longitude not

very different from 5^h west, but with no particular restriction, except that of gaining the highest possible south latitude. And the only point for inquiry would be, how well the Sun can usually be seen on December 6 at 2^h and 8^h Greenwich Mean Solar Time.

Royal Observatory, Greenwich, 1864, *May* 5.

Remarks upon the Statements of Messrs. Stone and Carpenter relating to Sir John Herschel's Figure of the Sinus Magnus in the Nebula of Orion. By Prof. G. P. Bond, Director of the Observatory of Harvard College.

(*Communicated by Mr. Carrington.*)

In the late Report of the Council of the Astronomical Society (*Monthly Notices*, vol. xxiv. pp. 92, 93) occurs the following passage relating to observations recently made at Greenwich upon the Nebula of *Orion*:—

"On the evening of January 11th the Nebula of *Orion* was splendidly seen. It was the opinion of the observers, Messrs. Stone and Carpenter, that the portions about the so-called jaws were not so accurately represented in the late drawing by Professor G. P. Bond as in that of Sir J. Herschel, printed in the *Results of the Cape Observations*, 1847. There is a squareness of outline about the so-called jaws, and an almost total absence of luminosity within them, in the drawing of Professor Bond, that certainly was not presented by the Nebula on this evening. The drawing of Sir J. Herschel appeared to represent, as accurately as perhaps any drawing could, the appearances presented about the jaws; the only marked deviation being that the wisp of luminous matter, 10^s following and 300 divisions north in Sir John Herschel's drawing, appeared to point more towards the trapezium than there represented. The other parts of the Nebula have not yet been examined with the same care as the portions about the jaws."

In offering a few words of comment upon these statements, I would remark at the outset that the drawing, for which I am responsible, is a proof taken from an *unfinished plate* of the Nebula about a year since. I have not, however, any desire to take advantage of this fact to screen the engraving from any censure which it may fairly deserve. Whoever attempts the impossible task of representing such an object faithfully upon paper will be certain to expose himself to the charge of inaccuracy in many important particulars, and just criticism is the surest means of advancing our knowledge of the subject; besides I am confident that the figure, as it stands, is capable in the main of a good defence. Its principal defect, in the

parts referred to, is much more in the relative intensity of the lights than in the drawing.

For example, Messrs. Stone and Carpenter find that "there is a squareness of outline about the so-called jaws, and an almost total absence of luminosity within them, in the drawing of Professor Bond, that certainly was not presented by the Nebula on this evening."

I find, on careful examination of a proof taken in the same stage of the plate, that barely *one-sixth* of the area within the "*fauces*" is represented as destitute of light, and that the proportion of surface illuminated in my drawing, instead of being less, is three times greater than in Sir J. Herschel's. I do not know of any figure of the Nebula which contains so small a proportion of absolutely dark ground as my own. The error into which Messrs. Stone and Carpenter have fallen I can explain only by supposing that they did not examine the engraving by daylight, or as closely by lamplight as they should have done. The fainter gradations of light are not there by accident, but are the results of a careful scrutiny of this region. The only areas quite destitute of light which I have found in this part of the Nebula are

1st. An irregular opening, with its centre in the position $\Delta\alpha + 108''$, $\Delta\delta + 50''$, and,

2d. A narrow channel having its axis nearly in the parallel and a declination of $\Delta\delta = + 72''$ at the right ascension $\Delta\alpha = + 160''$.*

It is perhaps an error to have left the light in this quarter so faint that it is liable to be overlooked, yet most of it is probably already too bright compared with other parts of the Nebula.

By the phrase "squareness of outline," I understand to be meant that the outlines have too much the appearance of straight lines, with abrupt angles at their intersections: in this sense I think there is room for improvement. At the same time any change made here for the better would affect the character and gradation of the light rather than the position of the outlines, which is the more important element.

I propose now to examine, with reference to some of the more prominent features concerned, the statement which follows the above, viz. —

"The drawing of Sir J. Herschel appeared to represent, as accurately as perhaps any drawing could, the appearances presented about the jaws; the only marked deviation being that the wisp of luminous matter, 10^s following and 300 divisions north in Sir John Herschel's drawing, appeared to point more towards the trapezium than there represented."

The positions given will be such as I have determined myself, unless it is otherwise stated.

* A positive sign to $\Delta\alpha$ and $\Delta\delta$ is used here, as elsewhere, to denote that the object follows θ *Orionis*, and is north of it.

Beginning with the southern shore of the *Sinus Magnus*, near the right ascension of the bright star H. 101, we have a first well-defined point of departure in the nebulous mass forming the following angle of the *Regio Huygeniana*, extending as far as the position

$$\Delta\alpha = +\ 145'' \qquad \Delta\delta = -\ 20''.$$

Of this abrupt terminus to the bright light immediately surrounding the trapezium there is no trace in Herschel's drawing. Nevertheless it is sufficiently conspicuous to have attracted the notice of Liapounov, although observing the Nebula at Kazan, under circumstances decidedly unfavourable. He has given for the position of this point (B), determined with the micrometer,

$$\Delta\alpha = +\ 146''\cdot5 \qquad \Delta\delta = -\ 22''\cdot0.$$

Herschel makes the breadth of the bright light here still 40″ to 50″, and continues the curve of the south preceding side of the Huygenian region for a distance of 180″ beyond its actual limit.

The direction of the southern shore of the *Sinus Magnus* varies considerably according to the gradation of light which is taken for its limit. The margin of bright light has a strong inclination to the parallel, to the amount of about 15° in the direction n. p. and s. f., which, however, would be variously estimated by different observers. Lamont and Liapounov have followed this margin. Herschel has taken the direction of the fainter nebulosity, which lies nearly in the parallel. In general, he has carried the bright light 10″ or 15″ too far north up to the vicinity of the trapezium.

Of the four prominences jutting out from the southern shore, forming intermediate bays, I have been able to recognise but one, that connecting with the "Bridge" (*Pons Schrœteri*), in the position

$$\Delta\alpha = +\ 84'' \qquad \Delta\delta = +\ 10''.$$

Much of the western shore is sharply defined, especially towards its northern extremity, where it is bounded by a brilliant mass referred to by many observers. This extends southward to the star H. 75, the latter being deeply immersed in the nebulosity, not on its edge, as represented in Herschel's engraving, where the shore of the gulf in this vicinity has been placed in too small right ascension, giving to the general direction of the western shore too large an inclination with the meridian, which the coast nearly follows.

The shore abruptly terminated along the bright mass referred to, passes through a point in the position

$$\Delta\alpha = +\ 30'' \qquad \Delta\delta = +\ 70''.$$

W. C. Bond, Lamont, and Liapounov, have respectively for the right ascension,

$$\Delta\alpha = +28''\cdot7, \quad +31'', \quad \text{and} \quad +30'',$$

each resting upon micrometric observations. Herschel's figure places this point in the right ascension

$$\Delta\alpha = +18'';$$

consequently 12″ too far west.

The "Bridge" connects with the northern shore precisely in the position given by Herschel. This feature should extend completely across the gulf to its southern shore. Near the middle it has a conspicuous bright spot in the position

$$\Delta\alpha = +70'' \qquad \Delta\delta = +44''.$$

Liapounov has

$$\Delta\alpha = +71''\cdot9 \qquad \Delta\delta = +42''\cdot3;$$

Lamont,

$$\Delta\alpha = +80'' \qquad \Delta\delta = +52'';$$

Herschel,

$$\Delta\alpha = +72'' \qquad \Delta\delta = +61''.$$

The remainder of the northern shore is very inadequately represented in Herschel's figure, both as to the locality of the leading features, such as the "*Nebula minima*," and the terminal "Cape" at the entrance of the gulf, as well as in the direction of the outlines.

From the meridian of the star H. 87 eastward, the margin of the *Sinus Magnus* exhibits, in the engraving, a depression which brings the shore as far north as the position

$$\Delta\alpha = +110'' \qquad \Delta\delta = +120'',$$

while its true limit is in the declination $\Delta\delta = +80''$, a deviation of 40″. A similar error of from 30″ to 40″ in declination affects all the neighbouring region, including the "*Nebula minima*" and the terminal "Cape." Farther north the limit of bright light is carried nearly 60″ from its proper locality.

The point which I have called the terminal "Cape" juts out boldly upon a very dark expanse, forming, with a telescope of sufficient illuminating power, a well-defined mark for micrometric measurements in declination. Its position is

$$\Delta\alpha = +150'' \qquad \Delta\delta = +83''.$$

Liapounov has

$$\Delta\alpha = +143''\cdot6 \qquad \Delta\delta + 71''\cdot5;$$

Herschel

$$\Delta\alpha = +152'' \qquad \Delta\delta = +116''.$$

Lastly, the coast-line in its vicinity, and the "*Nebula minima*," have a direction inclined by less than 5° to the parallel. The angle in the engraving amounts to 20°.

We find, then, as the result of the foregoing survey, the following instances of discrepancy between Herschel's delineation of the region in question and the actual appearance of the Nebula:—

1st. The absence of a definite limit to the bright light of the Huygenian Region on its eastern side, close to the southern shore of the *Sinus magnus*, and its consequent extension along this shore to a distance of 180″ beyond its proper terminus.

2d. The bright light on the southern shore is carried 10″ to 15″ too far north.

3d. Of the four prominences on this shore only one can be certainly recognised in the positions given.

4th. In its best-defined part the western shore is placed 12″ too far to the west.

5th. The bright spot in the *Pons Schræteri* is 15″ to 20″ too far north, and the bridge itself terminates without crossing the gulf.

6th. All the features of the northern shore to the east of the *Pons Schræteri* have been represented in positions 30″ or 40″ north of their true locality, at the same time that the direction of the principal lines is largely in error.

To comprehend the real significance of the discrepancies here indicated, the limited extent of the area in which they occur must be borne in mind. The distance between the opposite shores of the *Sinus magnus* is but about 80″, and its length 130″. Deviations of even less than 10″ are therefore far from being insensible or unimportant. They assuredly do not warrant the opinion expressed by Messrs. Stone and Carpenter, that the engraving "appeared to represent, as accurately as perhaps any drawing could, the appearances presented about the jaws."

I have felt justified in entering at such length upon the evidence in the case, both on account of the prominence given to the subject by its insertion in the Annual Report of the Society, and because it is really an important one in view of the questions which have been raised respecting the reality of changes in the aspect of the Nebula. It is only by attention to the more minute details of its features, especially as regards their locality, that these questions can be decided.

Observatory of Harvard College,
May, 1864.

Discussion upon Prof. Bond's Paper.

In the discussion which followed the reading of this paper, Mr. Stone remarked that the Astronomer Royal had been desirous last year of having a new drawing of the Nebula executed, in consequence, amongst other reasons, of a wish to that effect expressed by Mr. Otto Struve, who was under the impression that considerable changes had taken place in the Nebula. That drawing was entrusted to Mr. Carpenter, and he (Mr. Stone), while it was progressing, had a great number of opportunities of observing the Nebula with the large equatoreal. It was his impression, and also Mr. Carpenter's, that there was a squareness of drawing about the "jaws," and an absence of luminosity within the jaws, in Mr. Bond's drawing, which was not seen in the Nebula itself. He mentioned this in the Report, because if a change was actually taking place, it could only be verified by the comparison of a number of drawings taken at periods separated by considerable intervals, and it was essential, in order that the conclusions drawn might be satisfactory, that the drawings adopted as standards of reference for the different epochs, should not only be correct, but should be acknowledged to be so. If errors crept in and were allowed to remain, astronomers would, a few years hence, be subjected to great inconvenience, and would probably be misled.

From the acknowledged ability of Mr. Bond, his drawing will probably become the standard of reference for the present epoch, and he (Mr. Stone) thought it most important that all suspected deviations of that drawing from accuracy should be called attention to, in order that they might be settled now, whilst an opportunity of doing so was afforded us.

In reply to Mr. Hodgson, Mr. Stone remarked that he wished to raise the point about the reality of changes inferred from the comparison of drawings, and also that he believed that Liapounov's drawing did not lay claim to any great accuracy in the drawing of the details, but was chiefly valuable on account of the great care which had been taken in laying down the relative positions of the stars and a few salient points.

Dr. Winnecke confirmed Mr. Stone's statement with respect to Liapounov's drawing, and also stated that his impression in respect to the relative accuracy of Bond's and Herschel's drawings of the parts of the Nebula about the jaws, was identical with Mr. Stone's.

Mr. Stone remarked that it must not be taken for granted that Mr. Bond's engraving fairly did his drawings justice, bearing in mind the very great difficulty of representing faithfully in any engraving the actual appearance of celestial objects—Nebulæ especially.

The Rev. C. Pritchard, who fully concurred in this remark, stated that the Society had some exquisite drawings by Mr.

Lassell, but they were quite at a loss to know by what means to get them copied.

The benefit to be derived by calling in the aid of photography was then discussed, Mr. Vignoles strongly urging the desirability of such a step.

The Rev. C. Pritchard said he doubted whether the delicate shading of the Nebulæ could be brought out by photography.

Mr. Joseph Beck bore out Mr. Vignoles' statement as to the extreme accuracy of photographic copying.

Professor Selwyn brought before the notice of the Society 144 discs of the Sun, taken between Feb. 7, 1863, and Feb. 7, 1864, by the Ely Helioautograph. In connection with this subject he referred to a recent number of the *Astronomische Nachrichten*, which contains an account of Schwabe's observation of the spots for 1863. That indefatigable observer had numbered 124 groups in 330 observations. In the diagrams which were now before the Society, there were several interesting series to which he would call especial attention, extending from Jan. 24th to Feb. 1st (being an almost uninterrupted series of 8 days), from Feb. 11th to Feb. 18th, and from April 13th to April 24th. Professor Selwyn also exhibited to the Society some autographs of the Sun on a larger scale (6⅝ in. in diameter) made in accordance with the wish expressed by Mr. De La Rue and Mr. Hodgson. The production of these pictures was a work of great difficulty, and requires considerable perseverance, especially in getting the large telescope into working order. These drawings were taken on the 16th, 17th, 18th, and 19th May, and were provided with the lines of position, the necessity for which the Astronomer Royal had always so strictly insisted upon. That was the first series he had been able to take on this scale. The next was on the 23d May, and the last (and he considered it the best of all) was taken on the 1st of June, on which day a very magnificent spot appeared. The faculæ about this spot were clearly marked in the autograph, showing, he thought, that the chemical focus of the instrument had been very nearly found.

Professor Selwyn had not had time to deduce any conclusions from these observations, but he had no doubt they would be available as illustrations of the physical phenomena of the breaking out and closing up of the spots, quite independently of those more delicate questions which had been discussed by Mr. De La Rue and Mr. Carrington (whose recently published book was a perfect treasure of knowledge on the subject.) He then referred to two observations of Mr. Balfour Stewart's (who had the Kew instrument under his charge), which he thought would receive either confirmation or contradiction from such

autographs as those now before the Society. In examining the sun-pictures taken with the Kew Heliograph under the superintendence of Mr. De la Rue, Mr. Stewart has found that it appears to be a nearly universal law that the faculæ belonging to a spot appear to the left of that spot, the motion due to the Sun's rotation being across the picture from left to right.*

As far as the speaker had observed—but he was not in a position to speak positively on the subject—this result was not supported by the Ely autographs. He believed the faculæ surrounded the spots in the same way as the edges of a crater surround the cavity of a crater, not favouring one side or the other, but lying fairly round them. The other observation made by Mr. Stewart referred to the similar behaviour of all spots on approaching the same point of elliptic longitude, which was supposed to point to planetary influence.

Prof. Selwyn felt convinced that a continuous series of autographs could not fail to give the means of proving or disproving such statements. An analogy had been drawn between the solar spots and the cyclones which occurred upon the earth's surface. There were some interesting observations on the whirling of spots made by Padre Secchi, in the Memoirs of the Roman College Observatory. In his drawings there were clear indications of a resemblance between this whirling action of the spots and the cyclones or whirling storms of the earth. Sir J. Herschel had been the first to suggest this analogy, and he (Prof. Selwyn) would press upon all observers of the Sun these two questions. First, whether the whirling of the spots was in an opposite direction in the two opposite hemispheres, *i.e.*, *contrary* to the hands of a watch in the *northern* hemisphere, and with the hands of a watch in the *southern*, which had been observed to be the law of storms on the earth; and, secondly, whether there was a difference of temperature between the equatorial and the polar regions of the Sun. Padre Secchi, by means of observations with the thermo-electric pile, had arrived at the conclusion that there was an appreciable difference in temperature between the polar and equatorial regions.

* The following table expresses the result obtained:—

Year.	No. of cases of facula to left of spot.	No. of cases of facula to right of spot.	No. of cases of facula equally on both sides of spot.	No. of cases of facula mostly between two spots.
1858	2	0	0	0
1859	18	0	0	3
1861	9	1	3	0
1862	64	4	7	3
1863	47	0	9	2
1864	18	1	2	1

Reader, April 2, 1864, page 434.

The Rev. Mr. Howlett produced for the inspection of the members a portion of a series of spots on a very enlarged scale, embodying nine successive days' observations of one spot. He had observed, in connexion with this spot on the 25th January, certain phenomena, which, although on a minute scale, might be conceived of very great importance, as affording a link in a solution of the nature of sun-spots. He began to make a drawing of a spot at 11 o'clock, and observed at that time a tolerably condensed patch of umbral tint on the extremity of the penumbra. He drew it exactly on the margin. But by the time he had finished the spot, and others on the disc, upon looking over his work for the purpose of detecting errors, he found that the patch was no longer on the margin. Believing it to be a blunder, he altered it, but in an hour or so he observed that it was conspicuously removed from the edge of the penumbra. He watched it for another hour, when his impressions were put beyond a doubt, and he called in a friend to corroborate the observation. Both himself and his friend saw that the patch was distinctly drawing rapidly towards the central umbra, and by three o'clock in the afternoon it had advanced two-thirds of the distance = 12″ towards the great central umbra, at the same time becoming more condensed and elongated in the direction of the usual striations upon the penumbra.

Mr. De La Rue exhibited to the Society a diagonal eyepiece, constructed on the principle recommended by Sir J. Herschel, the second surface being at right angles to the pencil of rays falling on it, by which means the second reflexion is prevented. Mr. De La Rue also explained the adjustments necessary for its use with his large reflector. He congratulated the Society on the zeal which Prof. Selwyn had displayed in following out his solar photographs. He felt that the work now accomplished at Ely was extremely valuable.

Solar Eclipses observed in China from B.C. 481 to the Christian Era. By Mr. John Williams, Assistant Secretary.

I beg leave to lay before the Society the following list of Solar Eclipses observed in China from B.C. 481 to the Christian Era. They are recorded in a Chinese historical work, entitled *Tung Keen Kang Muh*, in 101 vols. sm. 8vo. This work contains a summary of Chinese history, from the earliest times to the end of the Yuen dynasty, A.D. 1368, and was first published during the Ming dynasty, about 1473. The edition

in my possession was published in 1808. The text is exceedingly brief, consisting merely of an account of the accessions and deaths of the emperors, and of the rulers of the minor states, with some of the more remarkable occurrences in each reign. There is, however, a commentary which supplies many of the deficiencies in the text. The appointments and deaths of other eminent personages are also noticed, together with the various great calamities that have occurred at different times, such as earthquakes, inundations, storms, &c. &c. To these must be added records of Eclipses and Comets, in the order in which they occurred. The present list of Eclipses commences where those recorded in the *Chun Tsew* cease. These are also given in this work, but are not included in the present list, having formed the subject of a former communication to the Society.

I must observe that the accounts are exceedingly brief, but little more than the year, season, and moon being given, and in several instances the year only: the day is in no case noticed. They are contained in the volumes of the work from 7 to 18 inclusive.

Some of the eclipses appear to have been total, but the word expressing totality used in the *Chun Tsew* occurs but once in this list. Another phrase is used which is rather ambiguous, but, from the context, in which it is stated that the stars were visible, it can refer only to a total eclipse. In two instances a second eclipse is said to have occurred in the month immediately succeeding the first; the words, literally rendered, being in both cases, "Winter, 10th month, Sun eclipsed; 11th month, Sun eclipsed."

The dates have been carefully verified by a comparison with a set of Chronological Tables, published about 1860 in Japan. These supply not only the Chinese Cycles and their years arranged according to their *Kea Tsze* system, but also the years in the European system answering to their Cyclical years. These Tables have been of very great assistance to me, as they can be fully depended upon, and, consequently, a great amount of time and computation has been saved.

I may also add that there appears to be a continuous series of Eclipses throughout the work. I have already traced nearly 100 additional ones from the commencement of the Christian Era to about the fourth century; and I purpose, from time to time, to lay before the Society additional lists, so as to give as complete a catalogue of Solar Eclipses observed in China as the means at my disposal will allow.

It is also remarkable that hitherto I have met with no records of Lunar Eclipses. Those of Comets are numerous, and there is also an account of a conjunction of the *Woo Sing*, or five planets in or near the Lunar division *Shih* (determined by α and β *Pegasi*), which appears to have taken place between 2400 and 2500 B.C. These may form the subjects of future communications.

Solar Eclipses recorded in the Tung Keen Kang Muh.

No.	Cycle.	Year of Cycle.	Season.	Moon.	Year B.C.	
1	XXXVI.	57	Summer	5	481	
2	XXXVII.	35	o	o	443	Total; stars seen.
3	,,	43	Summer	6	435	
4	XXXVIII.	8	o	o	410	
5	,,	21	o	o	397	
6	,,	36	o	o	382	Total; stars seen.
7	,,	43	o	o	375	
8	,,	49	o	o	369	
9	XXXIX.	57	o	o	301	Total.
10	XL.	50	o	o	248	
11	XLI.	34	Winter	10	204	
12	,,	,,	,,	11	204	
13	,,	40	Summer	6	198	
14	,,	50	Spring	1	188	
15	,,	,,	Summer	5	188	Total.
16	,,	52	Summer	6	186	
17	,,	57	Spring	1	181	Total.
18	,,	60	Winter	11	178	
19	XLII.	1	Winter	10	177	
20	,,	,,	,,	11	177	
21	,,	18	Summer	4	160	
22	,,	24	Spring	1	154	
23	,,	25	Winter	10	153	
24	,,	28	Winter	11	150	
25	,,	30	Autumn	9	148	
26	,,	31	Autumn	9	147	
27	,,	32	Winter	10	146	
28	,,	34	Autumn	7	144	
29	,,	35	Autumn	7	143	
30	,,	39	Spring	2	139	
31	,,	40	Autumn	9	138	
32	,,	44	Autumn	7	134	
33	,,	51	Spring	3	127	
34	,,	56	Summer	5	122	
35	XLIII.	6	Summer	4	112	
36	,,	25	Winter	10	93	
37	,,	29	Autumn	8	89	
38	,,	34	Winter	11	84	
39	,,	49	Winter	12	69	
40	XLIV.	1	Winter	12	57	
41	,,	4	Summer	4	54	
42	,,	16	Spring	3	42	

No.	Cycle.	Year of Cycle.	Season.	Moon.	Year B.C.
43	XLIV.	18	Summer	6	40
44	,,	24	Summer	6	34
45	,,	28	Winter	12	30
46	,,	30	Summer	4	28
47	,,	32	Autumn	8	26
48	,,	33	Spring	3	25
49	,,	34	Spring	2	24
50	,,	42	Autumn	9	16
51	,,	43	Spring	2	15
52	,,	44	Spring	1	14
53	,,	45	Autumn	7	13
54	,,	46	Spring	1	12
55	,,	56	Spring	1	2
56	,,	57	Summer	4	1

On the bright band bordering the Moon's Limb in Photographs of Eclipses. By G. B. Airy, Astronomer Royal.

In a communication to the Society, printed in their *Monthly Notices*, I pointed out, as the result of ordinary optical investigation, that no refraction through a homogeneous atmosphere surrounding the Moon could account for the appearance of brightness round the Moon's dark limb. This investigation includes the case of heterogeneous atmosphere; inasmuch as a heterogeneous atmosphere may be represented to any degree of accuracy by a succession of homogeneous strata with curved dividing surfaces, and the optical conditions are not altered by separating each stratum from the next by an extremely small quantity; and then the investigation above-mentioned is to be understood as to be repeated for every successive stratum: the view after the first stratum having been proved to be, in its character of brightness, exactly the same as if there had been no first stratum, it will follow that the view after the second stratum is the same as if there had been no first and no second stratum; and so on to the end. The phenomenon in question, therefore, cannot be explained by mere optical considerations.

Professor Stephen Alexander has, with great kindness, sent me specimens of the photographs taken by him in the Eclipse of 1860. In giving my account of the appearances of these, I must premise that I am furnished with numerous pairs of spectacles carefully adapted to very distinct vision at different distances, and that I have used the appropriate spectacles, or the unarmed right eye (whose refraction is very accurate), as circumstances required.

Upon first view of the photographs, the bright band is seen, rather conspicuously; and somewhat more conspicuous, as Professor Alexander remarked, in the neighbourhood of the cusps.

Upon viewing them at different distances, the spatial breadth of the bright band varies. When most distant from the eye, the band appears broadest (in linear measure). On bringing the photograph suddenly near (the proper alteration being made in the armature of the eye), the band seems to vanish; but, on looking more carefully, it is found to exist, but much narrower than before, and thus with intermediate degrees of breadth at intermediate distances. I could not pretend to estimate accurately its angular breadth, but, roughly speaking, it seemed to me to occupy nearly the same visual angle at all times.

I cut off a strip of the thin black photographic paper by a very clean line, and cemented it to the photograph very evenly, lying right across the middle of the bright lune. The other boundary-line of the strip of paper was very slightly ragged. The bright bands were seen on both edges of the strip, but rather sharper and narrower along the clean-cut edge than along the rougher edge.

These experiments leave no doubt on my mind that the phenomenon in question is a mere nervous irritation of the retina, produced by the view of the conterminous black and white portions of the photograph.

1864, *June* 9.

Shooting Stars in March. By A. S. Herschel, Esq.

In the year 1825 Brandes, in a work describing comparative observations of shooting-stars made at Breslau in 1823, remarked that shooting-stars in general proceed from that part of the heavens towards which the Earth is tending at the time. The place of this point upon the ecliptic, called by Herrick the tangential region of the Earth — perhaps better, the Apex of the Earth's Way* — is approximately 90 degrees of longitude behind the Sun. After comparing its average position with the course of 37 shooting-stars, determined in height and direction by himself, Brandes observes, "As far as a moderate number of observations can convince me, the planetary motion of the Earth upon its orbit occasions a prevailing shower of shooting-stars in the opposite direction." †

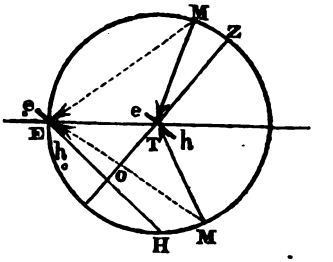
Herrick also observed in 1837 that throughout the month

* I am indebted for this term to the Rev. C. Pritchard, Sec. R. A. S.

† *Observations of Shooting Stars.* Leipzig, 1825. (p. 60.)

of November the shooting-stars at Newhaven (U. S.) pursued a prevailing direction from N.E. towards S.W. (*Silliman's Journal*, vol. xxxiii. p. 361). Continuing his observations through the winter, he added the remark that a most abundant season of meteors occurred on every night towards 6 o'clock A.M., and a season of least frequency at 6 P.M. Both of these effects he ascribed to the premises first stated by Brandes.

M. Coulvier Gravier wrote in 1845 (*Comptes Rendus*), "But for the cause which transfers, from the West to the East, nearly one-half of the number due to each of these directions, there would come exactly the same number of shooting-stars from the four points of the compass." The "cause" here intimated by M. Coulvier Gravier is the same as that formerly advanced by Brandes and by Herrick. It is ably elucidated by Mr. Bompas in the volume of the *British Association Reports for* 1858 (p. 144).



Suppose a small plane surface *eh* (fig.) to represent at once the horizon of an observer and the horizon of his Antipodes, upon the Earth, to the extent in which shooting-stars are visible to a single observer. The diameter of the circular plane *eh* will not in general exceed 200 miles (Herrick, *sup. cit.* p. 362). Let its centre T remain at rest for one second upon the trajectory of the Earth E T, at the point T: T Z perpendicular to *eh*.

If, for the purpose of calculation, shooting-stars are assumed to be cosmical particles, whose orbits intersect the orbit of the Earth at all possible angles of excentricity and inclination, *without any preference*, it follows that in general an equal number of meteors will fall upon the upper and lower surfaces of the plane *eh*, in the course of a single second.

Let the number be N, and suppose shooting-stars (on an average) to move with the velocity of the Earth in its orbit. An hour before their arrival at the plane *eh* the 2 N meteors will be found uniformly distributed upon the surface of a sphere E M H, of which the radius is 68,000 miles, the distance travelled by the Earth in an hour.

While the 2 N meteors approach towards the centre of the sphere, let the plane *eh* be carried by the Earth, from E to T along the trajectory way E T, so that when the small plane *eh* arrives at T the whole of the 2 N meteors may fall upon its two surfaces. There will fall more meteors than N upon the foremost surface of the plane *eh*, and less meteors than N upon the hindermost surface of the plane *eh*, *on account of the motion of translation of the plane.*

To know how many more meteors than N will fall upon the foremost surface of the plane eh in motion, let P represent the number, N representing, as before, the number of meteors which fall upon the same surface of the plane at rest. The proportion of P to N, or of N to P, may then be found by the following construction:—

E the Earth, and M any one of the 2 N meteors, one hour before their arrival simultaneously at T; $e_0 h_0$ a small plane (equal and parallel to eh), having its centre at E. Produce the plane $e_0 h_0$ until it intersects the surface of the sphere E M H in the small circle E H. The larger segment of the sphere is upon the foremost side of $e_0 h_0$. Because the meteor M approaches E (in motion) along the chord M E, the whole of the meteors upon the segment E M H will at the end of the hour fall upon the foremost side of the moving plane $e\,h$.

But on the supposition that the orbits of the 2 N meteors intersect the orbit of the Earth at all possible angles of excentricity and inclination—*without preference*,—the number of meteors on the segment of the sphere, E M H, is directly proportional to the area of the segment. Therefore the number P bears the same proportion to the number N which the area of the surface of the spherical segment E M H bears to the area of a hemisphere.

$$\begin{aligned} P : N &:: \text{area of segment E M H} : \text{area of hemisphere} \\ &:: ZO : ZT \\ &:: \text{versed-sine } ZTE : 1 \end{aligned}$$

$$\frac{P}{N} = \text{versed-sine of Nadir-distance of the apex of Earth's way} \qquad (1)$$

Having observed the number P, which is the number of meteors per second when the apex of the Earth's way has a known angular distance from the observer's Antipodes, and having divided this number by the versed-sine of the angular distance in question, the number N obtained as the quotient of this division is the number of meteors which would have fallen upon the observer's horizon, or upon the same small plane of view *per second, had the Earth remained at rest.*

Similarly (disregarding the change of Nadir-distance of the apex of the Earth's way), if W is the value of that angle at the middle of an hour when P meteors have been observed, the number *per hour, for a stationary horizon,* becomes

$$N = \frac{P}{1 - \cos W} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If shooting-stars (on an average) move with $v \times$ the velocity of the Earth in its orbit, and W is the Nadir-distance of

the apex of the Earth's way, the correction is made by the equally simple formula,

$$N = \frac{v}{v - \cos W} \times P \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

of which the former equation (2) is a corollary obtained by making $v = 1$.

There are, in all probability, 6150 independent horizons, or small planes of view, for shooting-stars, upon the whole spherical surface of the Earth, including sea and land.* If, therefore, N be the number of meteors which would fall upon any single one of these areas (could the Earth be held at rest in its place for one hour), $6150 \times N$ would represent the whole number falling in the same time upon the globe at rest. Suppose this number to be M, then,

$$M = 6150 \times \frac{v}{v - \cos W} \times P \quad . \quad . \quad . \quad . \quad (4)$$

The number M, calculated from the observed number P, the known angle W, and a hypothetical value of v is a correct measure of the scarcity or abundance of meteors at different points of the Earth's orbit. With the exception of certain extraordinary dates, it is probable that M is sensibly constant throughout the year. Writing $\frac{M}{6150} = 1$, equation (4) becomes

$$P = 1 - \frac{\cos W}{v},$$

or transforming the equation

$$P = 1 + \frac{\sin l \sin \Delta + \cos l \cos \Delta \cos H}{v} \quad . \quad . \quad (5)$$

in which l is the North latitude of the place of observation, H and Δ the hour-angle and North declination of the apex of the Earth's way.

From this expression it is seen that, M being constant, a daily *minimum* of meteoric frequency is attained at 6 P.M. (when $H = 180°$), and a daily *maximum* of meteoric frequency at 6 A.M. (when $H = 0$).

At midnight, when $H = 90°$ (approximately),

$$P = 1 + \frac{\sin l \sin \Delta}{v} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and it is seen that, M being constant, the frequency of meteors

* Herrick, *American Journal of Science*, vol. xxxiii. p. 362.

at midnight ranges through the year (*in northern latitudes*) from

$$P_0 = 1 - \frac{\sin 23^\circ 28'}{v} \cdot \sin l \text{ (in March) to}$$

$$P_{,} = 1 + \frac{\sin 23^\circ 28'}{v} \cdot \sin l \text{ (in September)} \quad . \quad . \quad (7)$$

If, for example, $v = 1$, the extremes of frequency in *northern latitudes from March to September* are expressed by the formula

$$P = 1 \mp \sin 23^\circ 28' \cdot \sin l \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and the extreme range of frequency

$$= 2 \sin 23^\circ 28' \cdot \sin l \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In the latitude of London, $l = 51^\circ 32'$,

In March	$P_0 = 0{\cdot}688$
In September	$P_{,} = 1{\cdot}312$

and these numbers are to one another in the proportion of 1 : 1·894.

It is remarkable that at Düsseldorf, of which the latitude differs only 19′ from that of London, this proportion of the extremes was observed by Benzenberg in 1837. Examining a single quarter of the sky on 26 ordinary nights, Benzenberg observed 1·9 shooting-stars per hour to be the average midnight frequency in spring; and 3·6 shooting-stars per hour to be the average midnight frequency in autumn.* These numbers are to one another in the proportion of 1 : 1·906, agreeing closely with the calculated ratio.

It is seen from (8) that, M being constant, the range of meteoric frequency throughout the year at midnight diminishes with the latitude. It is perhaps on this account that no seasons of meteoric scarcity are noticed by eminent travellers in tropical countries. Humboldt expresses himself as follows: "It appeared to me that meteors were more abundant than with us. This was not improbably, however, a deception which would disappear before a strict enumeration." Meteors appear to be equally frequent throughout the year in the low latitudes of the peninsula of India, and a season of scarcity is less perceptible even in the United States of America than

* Benzenberg, *Falling Stars*. Hamburgh, 1839, pp. 201–3.

in Europe, apparently on account of the smaller distance of these continents from the equator.

Not only the ordinary kinds of shooting-stars, but fireballs and aerolites, and the extraordinary displays of shooting-stars called star-showers, are more frequent in autumn than in spring. In Mr. Greg's Catalogue of 1800 Fireballs and Aerolites, the division of the year into equal portions (from January to June, and from July to December), is found to separate the occurrences of large meteors into a season of scarcity followed by a season of abundance, in the proportion of 739 to 1059 meteors, or of 1 : 1·433.* In this Catalogue the latitudes of Europe predominate among the localities. Seasons of scarcity and abundance appear with the same division of the year in M. Quetelet's Catalogue of Star-Showers (*Physique du Globe*, Brussels, 1861, p. 312), in the proportion of 28 to 72, or of 1 to 2·571. It should not be forgotten that the long and the short nights of the year are equally divided between the seasons in question, both in this instance and in the case of fireballs.

The account hitherto given of the two seasons of meteoric frequency is briefly the following:—"The Earth encounters more meteors in going from aphelion to perihelion, than in going from perihelion to aphelion."†

If the cause has been rightly traced (after the principles of Brandes, Herrick, and others) to the planetary nature of the Earth, the obliquity of the ecliptic, and to the cosmical theory of shooting-stars, this concise description of the late celebrated French astronomer Arago will require to be inverted in the Southern Hemisphere.

Shooting-stars not unfrequently cross the circle of the Sun's longitude from west to east; but their course is then protracted, and their light is red and dull, as might be expected from their diminished speed with relation to the Earth. Such shooting-stars are seen at all times of the year, but most frequently in *March*, when the apex of the Earth's way is depressed below the horizon. They serve, in the absence of Southern observations, as further confirmations of the foregoing principles of Brandes and Herrick.

March 31, 1864.

* *British Association Report for* 1860, p. 116.

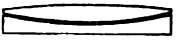
† Arago, *Popular Astronomy* (transl.). London, 1858. Vol. ii. pp. 495, 500, 505, 509.

The Achromatic Object-Glass. By R. Hodgson, Esq.

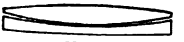
With the desire of exciting some further examination into the Achromatic Object-Glass, I have ventured to draw the attention of the Society to the various forms used by different opticians to obtain the same result, and while the form adopted by English opticians is the same as that used by the inventor John Dollond, the foreign opticians have made wide and large variations, as shown in the diagrams.

No. 1.
John Dollond, 1758.

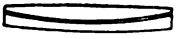
No. 2.
Peter Dollond, 1765.



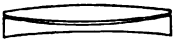
No. 3.
German.



No. 4.
Frauenhofer.



No. 5.
Sir John Herschel, 1821.



No. 6.
Professor Barlow, 1827.

No. 7.
Steinheil, 1860.

Nos. 1 and 2, the forms of the inventors John and Peter Dollond, followed by Tulley, and their successors, in double and triple object-glasses; then the German forms, Nos. 3 and 4, as applied by Frauenhofer; and now used by the French and German opticians, for the largest telescopes. The form of Sir John Herschel, No. 5, very nearly the same as Frauenhofer. Professor Barlow, No. 6, and the last, No. 7, is that of a new object-glass by Steinheil of Munich, computed from a paper by Gauss, published as long since as 1817; and from some remarks by Mr. G. P. Bond, the American astronomer, it would appear that this form admits of an increased aperture for a given focal length or a diminished focal length, with a given aperture; the light increasing in both cases.*

The able article, "Telescope," in *Rees' Cyclopedia*, by Dr. Pearson, and the paper by Prof. Barlow, in the *Phil. Transactions*, 1827, probably form the bases of computation of all the large English object-glasses made during the last few years. In the paper by Sir John Herschel (*Phil. Trans.* 1821) are several rules and formulæ; one very similar to that of Steinheil, which he characterises thus:—

"The values of the first set are, *however, objectionable*, as they will be found to correspond to meniscus and concavo-convex forms of the crown and flint lenses respectively, of great curvature, and placed together."

* In the recent additions to the Library of the Society, from the Turnor fund, is a curious old folio volume, "Zahn's Optics," 2d edition, printed at Nuremburg, 1702, containing a great many diagrams, amongst them the following:—

Whether the object-glass of three convex surfaces and one concave was first used in England or Germany is a doubtful point; but, upon comparing the triple object-glass, which has the two exteriors convex, it appears an easy transition to abandon one of the crown lenses and reduce the surfaces again to four, retaining the two exteriors convex.

Mathematicians and opticians have always deplored the want of good materials for object-glasses, and, although the glass at present made cannot be called perfect, yet, in comparison with that manufactured twenty or even ten years ago, it must be admitted to be excellent, and, with the improvement in material and the application of steam power to machinery for grinding and polishing, it would be satisfactory to know that the form of the English object-glass is the best that can be used. The regularity or stability of the glass now made by Messrs. Chance of Birmingham (the variation probably being within a range of from three to five per cent) would, perhaps, admit of a few short tables being computed, to enable the working optician to make at all times a good object-glass, according to rules laid down, instead of labouring in the dark by trial and error to arrive at what too often is but an indifferent result.*

* The object-glass produced at the Meeting, of 4 inches aperture and 63 inches focal length, was made in 1849, after a careful reading of Prof. Barlow's paper. The refractive and dispersive values of the glasses were found from small lenses made of similar glass, according to the directions laid down. The curves were then calculated, and the tools made to the respective radii. The four sides were each only *ground once* and *polished once*, and the object-glass will on favourable nights show the five stars in the trapezium of *Orion*, and all other stars usually seen with good telescopes of 4 inches aperture.

On μ Herculis. By the Rev. W. R. Dawes.

I have the pleasure of announcing the curious fact, that the small star *s. p. μ Herculis*, which in the year 1856 was discovered by Mr. Alvan Clark to be close double, is a *binary* system; and that within the last five or six years the variation in position, as deduced from my own observations, amounts to about 18°. Thus the resemblance of this triple system to that of *μ Boötis* is complete. As I remarked in my notes appended to the first series of new double stars discovered by Mr. Clark, it was ascertained many years ago by Professor Struve that this small star has a common proper motion with *μ Herculis*; as is also the case with *μ Boötis* and the small double star which accompanies it.

I measured the companion of *μ Herculis* twice in the year 1857, but found it extremely difficult. In 1859 I obtained

three sets of measures more easily, and of better quality, with my present telescope. Allowing the weights assigned to the different sets, the mean comes out,

P = 59°·91; obs. 32; w. 116 : D = 2″·025; obs. 20; w. 50; epoch 1858·77.

The night of the 6th of this month being very fine, I turned my telescope upon this object, and was instantly struck with the large variation in the angle. This was made so readily apparent by the fact, that in 1859 the close stars pointed almost precisely to μ; but at present the line joining their centres makes an angle of 15° or 16° with the line joining μ and the brightest of the close pair.

The result of the single night's measures is,

P = 77°·59; obs. 5; w. 32 : D = 1″·806; obs. 6; w. 17; epoch 1864·43.

Comparing these with the mean above quoted, we find an increase of the angle amounting to 17°·68 in 5·66 years, or $3\frac{1}{8}$° in one year; while the distance has probably not varied perceptibly.

As the position and distance of the smaller star with respect to μ were observed by Struve at Dorpat on six nights in three different years, and at Poulkowa so lately as 1851, without the duplicity of the companion being perceived, it seems almost necessary to conclude that the distance must have been very much less at that time. Yet considerable doubt is thrown upon this by the fact that, if my observations may be relied on, no increase in the distance has taken place since 1857.

On the same night (June 6) I obtained good measures of *Antares*, which gave

P = 175°·71; obs. 5; w. 28 : D = 3″·675; obs. 10; w. 30; epoch 1864·43.

The small star was frequently well seen, and quite free from the light round its brilliant neighbour. There seems at present to be no certain proof of any change having taken place in this star; but the observations, on the whole, point to a slight increase of both the angle and the distance.

Hopefield Observatory, Haddenham, Bucks,
1864, *June* 8.

On the large Sun-spot Period of about 56 *Years.*
By Balfour Stewart, M.A. F.R.S.

Besides the well-known period of 11·2 years, Prof. Wolf has found, both fron sun-spot records and from those of Auroræ, a longer period of about 56 years, which had its maximum in 1836.

From the recent comparison instituted by Mr. Carrington we have grounds for supposing that a connexion may, perhaps, exist between the radius vector of *Jupiter* and the development of Sun-spots, such that, when *Jupiter* is furthest from the Sun, there are most spots. If this be true, it is reasonable to suppose that, if at any epoch both *Jupiter* and *Saturn* came to aphelion together, we ought then to have had a remarkable development of spots. No doubt both of these planets are far from the Sun; but, on the other hand, they are of great mass and of considerable excentricity.

Let us suppose that we had such an epoch now, since two revolutions of *Saturn* are very nearly equal to five of *Jupiter*, 59 years afterwards we should have the same planetary phenomenon repeated, and also another remarkable development of Sun-spots. So far this accounts very well for Prof. Wolf's period of 56 years.

Let us now examine the dates of aphelion of these two planets, and find at what epoch, according to our hypothesis, we ought to have had a maximum of spot-frequency.

In the following table these dates for both planets are given and compared together:—

Date of Aphelion of Jupiter.	Date of Aphelion of Saturn.	Difference.
1815·4	1812·1	+3·3
1839·2	1841·6	−2·4
1874·9	1871·1	+3·8

We thus see that the dates of aphelion of the two planets come closest together about 1840, which is not far distant from 1836, the date of maximum given by Prof. Wolf.

New Minor Planet. By N. R. Pogson, Esq.

(*Extract from a Letter to the Astronomer Royal.*)

"I think the identity of my last new planet with *Freia* is now quite established, and the name *Sappho* again at liberty for future use. It was one which Sir J. Herschel had suggested to me as suitable for a small planet, and hence my preference for it. I have, therefore, much pleasure in providing another candidate, the first being disqualified; but this one most certainly and evidently new. It was first seen on May 2 and mapped as a star, but its planetary nature was not suspected until the following night.

"Its partially reduced positions then, and its approximate last recorded place, are

	Madras M.T.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
May 3	13 44 11	16 12 3·4	106 47 11
	14 44 4	16 12 0·9	106 46 55
12	12½	16 4 9	105 46 0

Magnitude 10·4 Daily Motions − 53ˢ, + 6¾′.

"*Freia* was observed on twenty-five nights: this one, so far, on four.

"*Madras*, 1864, *May* 14."

Mr. De La Rue reports most favourably of the performance of a telescope of new construction sent by Dr. Steinheil for exhibition at his reception held at Willis's Rooms on June 11th. The objective has an aperture of 48 lines (about $4\frac{2}{10}$ inches English) and a focal length of only 40 inches (French). It is not the objective of Gauss, already made public. Mr. W. T. Radford has purchased the telescope, so that it will remain in England. Dr. Steinheil is now engaged upon an object-glass composed of three lenses of 6 inches aperture, and only 30 inches focal length.

From a communication received from M. Le Verrier, it appears that his meaning with respect to relative value of eye-and-ear and touch observations, has been in some measure misapprehended. M. Le Verrier's only intention, in the conversation reported in the May Number of the *Monthly Notices*, p. 159, was to express that, in his opinion, a touch-observer does not acquire the same appreciation of duration of time as an observer trained to eye-and-ear observations.

Although the discussion at the Meeting appears to have been correctly reported, the Council regret that they have given publicity to a citation of an oral conversation, which conveys to some extent an erroneous statement of the views of the illustrious Director of the Imperial Observatory of Paris.

ERRATUM.

In Monthly Notices, No. 7.

Page 165, line 12, *after* the Rev. W. R. Dawes, *insert* that their existence is is at least doubtful.

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MONTHLY NOTICES

OF THE

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VOL. XXIV. *Supplemental Notice.* No. 9.

On certain Suspected Changes in the Lunar Surface.

By the Rev. T. W. Webb.

(*Communicated 13th May, 1864.*)

It is now precisely five years (even to the day), since I had the honour of submitting to the Royal Astronomical Society a notice of traces of eruptive action in the Moon, in which I drew attention to the spots *Cichus* and *Messier*, as furnishing probable evidence of the continuance of explosive force during the present century. I had hoped, in the intervening time, to have entered somewhat further into this curious subject; but various hindrances, among which may be especially mentioned the prevalence of cloudy skies, have combined to interfere so materially with my purpose that, even at this distant period, I have but little to communicate. Small, however, as my contribution to Selenography must appear, I beg permission to offer it to the Society, under the impression that, in a work of such extent as the delineation of the Lunar Surface, every additional fact, even if insignificant in itself, possesses a certain amount of relative value.

Being unable to adduce any evidence of change from my own observations, and, as it happens, not having hitherto particularly studied the portions represented in the Sections of Lohrmann, I have uniformly referred, for the purpose of comparison, to the great Map of Beer and Mädler. In doing this, I have been obliged to notice, with concern, occasional indications of a want of that high accuracy which might have been expected from the general character of the work. I have never entertained a doubt as to the correctness of their triangulation, or of the laying down of such primary points as are immediately connected with it; but it has appeared to me that an equal amount of dependence cannot always be placed upon the subsequent filling in, by mere eye-estimate, of lesser details. This is much to be regretted, not only as diminishing in a general sense the confidence with which every observer would wish to regard this truly great undertaking, but as disappointing us more especially in that very respect in which a reliable standard of comparison would have been so desirable. It seems generally admitted that no alteration of any considerable magnitude has taken place upon the lunar surface since

the date of anything which can be called accurate observation; it is only in the smallest class of craters, therefore, that we can reasonably look for traces of continued activity; and it is precisely *there* that we become sensible of a deficiency in the work of the illustrious German astronomers; in some instances we find these minute details laid down with less regard to accuracy of position than would have been desirable; in others, we are inclined to question whether they actually represented all that their instrument was capable of showing them, and that they, therefore, might have seen, especially if they had been willing to avail themselves of the labours of their predecessors. It is, in fact, difficult to reconcile the extraordinary pains and diligence which characterise the larger part of this grand undertaking, with the incorrectness which mars some of its details; it is very difficult to understand how it should have been done so well, and yet not have been done better. It would be easy to give proof of what has now been asserted, especially in reference to a comparison with older delineations: we have one remarkable instance in the great plain recently termed by Dr. Lee the *Mare Smythii*, where Schröter's figure, under the name of Abraham Gotthelf Kästner, is much more faithful than their own; and another, in the *Sinus Aestuum*, where their assertion that no craters are discernible was afterwards retracted by Mädler, and never, one would suppose, could have been made had they consulted the design of Lohrmann. In these and similar cases, the accuracy of their map seems to have suffered from their unwillingness to be under obligation to those who, if in some respects inferior to themselves, had a right, as credible witnesses, to a fair share of attention. From the persevering investigations of Mr. Birt it would appear that their deficiencies are very obvious in the regions adjacent to the limb, where, it may be admitted, the difficulty of delineation was greatest: my own observations, referring to more central regions, where errors of such magnitude would naturally not occur, still indicate, from time to time, want of care in the representation of minute features. And, though allowance must be made for the superiority of my instrument, this cannot be extended very far, as the extreme minuteness and perfection of their detail in many cases show what might have been attained in all. It is the unfortunate want of confidence in our highest standard thus necessarily resulting which throws some doubt upon the evidence of change which I am about to adduce, and renders an appeal to the future still necessary to render it conclusive.

Of the various observations in my journal, some relate to objects so minute that, though I have carefully recorded them, on account of their possible prospective value, they find no place in the present inquiry; others are not satisfactory to myself, or require confirmation: a few only have been selected, which seem the best established, and which I feel the least hesitation in recommending to the attention of other observers.

I. *Helicon* A, according to Beer and Mädler's nomenclature, or *Leverrier*, as it has been designated by the French astronomers.

As far back as 1836, May 26, I had noticed, with an achromatic telescope of $3\frac{7}{10}$ inches aperture, and a power of 144, a crater which I then described as "extremely minute, about the *minimum visibile* with the present weather and magnifying power," lying close to the ring of *Leverrier* on its S.E. side.

1861, April 20, after I had completely forgotten the previous observation, I found the same object, "very conspicuous," owing, perhaps, to the greater power of my present telescope; and since that time I have repeatedly seen it with perfect ease.

II. *Helicon.* At the date of my rediscovery of the last object, "I was at once struck," as I have expressed it in my journal, "by the unusual appearance of a minute, but unquestionable, crater on the summit of the E. wall of *Helicon;*" and I have, on many subsequent occasions, found it very visible; and even been able once to make out the shadow in its interior; it is entered as "most obvious" when the terminator was passing through *Aristarchus;* and in one evening it was seen even when that line was so remote as to include *Schickhard.*

III. Crater in the *Mare Imbrium.* In the direction from *Leverrier* towards *Archimedes,* a little N. of the letter b in the word "Imbrium" in Beer and Mädler's Map, is a small crater, which they have not inserted, notwithstanding its distinctness and insulated situation in the midst of the open plain. A little marking, which catches the eye on the map near its place, will be found, on closer examination, to be nothing more than a defect in the engraving. I first noticed the crater, 1861, May 18, when it was more conspicuous than several small craters in the neighbourhood figured by Beer and Mädler; and I have since seen it on other occasions without difficulty.

IV. *Kirch.* Between this object and a similar crater to the N.W., marked A in the map, and referred, as it appears, to *Pico* as head-quarters (why, it is not evident), I found, 1861, Dec. 10, a minute but very obvious crater of an elliptical form, containing a distinct shadow. This is only one of several omissions in the same neighbourhood, but the rest, like other minute craters in the *Mare Imbrium,* are so small that they cannot be fairly adduced in evidence.

V. *Delisle* b, a crater of moderate dimensions in a line between *Carlini* and *Wollaston,* has a small crater on the N. end of the ring, not to be found in the map, notwithstanding its obvious aspect and conspicuous situation. I first noticed it, 1863, September 23.

VI. *Diophantus.* Just S.E. of this object lies a very visible little crater, omitted by Beer and Mädler, but observed by myself, 1863, September 24.

VII. *Copernicus.* 1862, Feb. 8, I found the S. and S.W. slope of this magnificent ring thickly studded with very minute

craters, not represented by Beer and Mädler. The omission is chiefly remarkable, as they form a continuation of the extraordinary assemblage of similar foci of eruption lying between *Copernicus* and *Eratosthenes*. This latter wonderful district, it may be observed by the way, has probably assumed its present honeycombed aspect during the present century, as it is hardly conceivable that in such a situation it should have escaped the persevering scrutiny of Schröter, and been left for the eye of Gruithuisen in 1815.

VIII. *Mersenius*. This ring, as is well known, is remarkable for the very unusual feature of a convex interior, which was discovered and well drawn by Schröter, and appears then to have had no object upon its summit. It is represented equally smooth in the Map of Beer and Mädler; and, from their description, we are assured that this was no error in the drawing, for they speak of several inconsiderable ridges and terraces round the inner foot of the ring, and a general upheaving of the interior towards the centre, so that for some hours this bubble-like convexity casts a distinct shadow to the eastward in the increasing Moon, and they have, though opportunities are not so frequent, seen it falling in the opposite direction; but they expressly add that "a central mountain is totally absent." No further evidence can be needed to show that they had thoroughly investigated the spot. Now, in the year 1836, May 27, I perceived with my smaller achromatic, on the summit of this well-known convexity, a minute crater and several other delicate markings, which I never thought of subsequently till 1855, Oct. 22, when I caught an indistinct sight of them again. 1863, Sept. 24, I had a much better view of them, though the air was not very favourable; they had disappeared on the following morning; and I have no doubt they may at any time be readily verified under suitable illumination with an instrument of moderate power. The aspect of a considerable portion of them is that of a line of long irregular ravines without elevated margins, as though they had been formed by the dropping in of part of an inflated and hollow crust. We have here something that certainly looks like evidence; and it would seem as though the epoch of the supposed convulsion might be fixed within narrow limits; for the Map was published between 1834 and 1836, and the letterpress in 1837, both probably from sketches and memoranda of a somewhat earlier date; while my first detection of these objects took place in 1836. Gruithuisen speaks of a small cleft on the central convexity of *Mersenius* in a work published in that year; but gives no date of observation, merely including it in his general remarks on a number of similar features, with some of which he seems to have been long familiar.*

* I have since found that Lohrman has inserted a minute crater on the convexity in his map, made from observations taken between 1822 and 1836. —T. W. W.

The foregoing instances seem worthy of notice, from their sufficiently, and in several cases peculiarly conspicuous situation, in juxtaposition for the most part with objects well known and frequently observed. Some of them were detected with an instrument inferior to that employed by Beer and Mädler; and I should not have called attention to any of them unless they had been of greater magnitude and visibility than other features which they have faithfully represented. Now, in order to account for these presumed changes, it has appeared to me that only three explanations are possible. However singular may be the variations of aspect due in certain cases to libration, the objects which have been mentioned are so well marked out, and so readily and permanently visible, that their non-appearance cannot in fairness be referred to that cause: a little experience in lunar observation would convince us that they do not belong to a class whose inconstancy of appearance can be explained in that way. It remains that they must be accounted for, as far as I can see, either by the supposition of inadvertency on the part of former selenographers,—of actual physical alteration of surface,—or of some kind of obscuration arising from eruptive action, or from the changed conditions of a possible lunar atmosphere,—an alternative due to Schröter, who frequently had recourse to it, in order to explain appearances, of which it seemed to him that no equally reasonable account could be given. Any other supposition, besides these, I confess that I have been unable to devise. Nor do I presume to offer an opinion as to the comparative probability of those I have mentioned. I would prefer to leave the subject in the hands of those better qualified by superior instrumental means and longer practical experience to form a satisfactory judgment. But, before bringing these remarks to a close, I would beg to direct attention to one more singular instance of variation, which sets in a very strong light the difficulty and uncertainty attendant upon these interesting researches. It will be that of the well-known object—

IX. *Gassendi.* This great walled-plain would naturally, from its position, attract the attention of all selenographers; it was twice delineated by Schröter in different circumstances of libration and illumination; and in the Map of Beer and Mädler it has all the appearance of careful drawing, while the corresponding description is unusually full and precise. They speak of an interior containing in the Full Moon two rows of luminous specks, whose junction is marked by a bright spot, which they measured for position on nine several occasions. They then tell us how, as the illumination declines, this spot disappears, together with the majority of the other specks, and a central mountain only four or five miles S. of it comes into view; while, when the angle of illumination is very small, the same spots return again as mountains, not in the previously described form, but as longish ridges stretching in various directions

among a great number of smaller irregularities in every part of the surface, so that any one who had not watched *Gassendi* attentively from night to night would never recognise, in a certain little inconspicuous hillock, the glittering point prominent in the Full Moon, but would rather be inclined to suppose some physical change: and yet, they add, all this ensues, as we have described it, in every lunation. With all this minute recital before him, an observer, who, like myself, found, on the evenings of Sept. 23, 24, and 25, in last year, a minute, but brilliant, crater standing out so clearly and sharply upon the floor, that there could be no possible doubt of its nature; and on Oct. 23, a second of the same dimensions near it, perfectly decided and unquestionable, might well have been excused for supposing that actual outbursts must have occurred since 1837 in this thoroughly explored region. What, then, would be his surprise to find that he was quite wrong in this idea, and that these two small craters had both been seen and drawn as far back as 1797, by Schröter, who himself expresses his wonder that he had never noticed them before, even in using a larger instrument, during the observations of nearly four years?

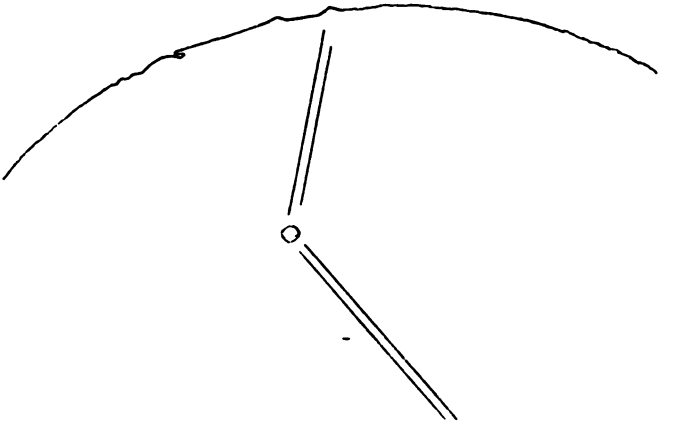
I will not occupy the time of the Meeting by a description of other singular and perplexing appearances which I have met with in the interior of this walled-plain, especially as the express investigation of it is known to be in better hands. Nor is there any occasion for a lengthened comment upon this curious instance of alternate visibility and invisibility. Insignificant as it is in point of magnitude, it will be seen at once to be eminently suggestive of the necessity of caution in drawing inferences from appearances with which we have not been long and thoroughly familiar. We would not press it too far against the probable evidence of changes in our own time, when we are well assured that they were frequently taking place at some former time, and can assign no sufficient reason why they should not be still in progress; but it may teach us the danger of forming precipitate conclusions; and we may add that the observer who will succeed in giving a satisfactory explanation of these discrepancies will have made no small advance in clearing up the difficulties of selenography.

Account of his Observations at Malta. By W. Lassell, Esq.

(*Extract from a Letter to the Rev. C. Pritchard.*)

On the 21st instant, about 8 P.M., G.M.T., the atmosphere not being fine enough for advantageous use of the large telescope, I went into the small dome, in which I have recently erected the 9-inch Equatoreal, originally mounted at Starfield,

many years ago. The large speculum, however (9·1 inches diameter and 112 inches focus), now in the tube, was refigured only about five years since. On turning this instrument on the Moon, I believe with no more precise object than to learn the state of the atmosphere with this aperture — an eye-piece magnifying 199 times having been applied—I was immediately struck with an unusually irregular outline of the Moon's limb, and was reminded of a rather startling diagram I had seen in a paper by the Rev. Henry Cooper Key, in the *Monthly Notices*, vol. xxiv. page 20. I made a drawing at once in my journal, of which this is a fac-simile:—



A careful comparison afterwards of the diagram with the Moon convinced me that it might be considered optically correct. The position of *Tycho* and two of the principal lines radiating from it, given in the diagram, will point out pretty nearly in what part of the Moon's limb these irregularities were observed. But it will be seen that in proportional magnitude they are *very much* less than those described by Mr. Key, the length of the flat portion — (I am not sure that it was really *concave*)—not extending over more than about 11 degrees of latitude. As the definition of this telescope always excites my admiration, and is, indeed, not surpassed by that of any telescope of any form that I have seen, I think that no "irradiation" could have a sensible effect "in obliterating the true outline" of the Moon's limb. It appears, however, that very different parts of the limb were observed in the two instances.

While speaking of this telescope, may I be pardoned if I say a word or two as to its quality; for I have not been so long familiar with large telescopes as to make me insensible to the merits of small ones.

The "sixth star" of the trapezium of the nebula in *Orion* is easily seen with it by an ordinary eye with powers from 200

to 300. I saw it best on the 3d March last with a single lens magnifying 291 times. *Enceladus* is well seen when near its greatest elongation, in the present position of *Saturn's* ring,—about as easily as *Mimas* is in the 4-foot Equatoreal; unless the air be very tranquil, when the latter has the advantage. The small star near δ *Cygni*—that severe test for all telescopes—exhibits a palpable disk, not to be overlooked by a casual observer, even in a barely average state of atmosphere. I have seen it best when the air was quiet, with a power of 471. The very minute and faint double star μ^2 *Boötis* it now separates by the finest line, under favourable circumstances, with a power of 732.

The companion of *Antares* is too obvious to form a test. I see it with almost any power. I do not know the history of this star, but I presume its components must have separated since its discovery. On two evenings, when the air seemed favourable, except some "moulding"—which I have often noticed when in a dome the telescope is pointed near the zenith—I placed the telescope upon γ *Coronæ;* but the star appeared round even with high powers, and I could gain no impression of duplicity. On turning the 4-foot upon it, however, I found I had no right to expect to see it with the smaller instrument; for, with a power of 1060 on this, it appeared a considerably unequal double star, the disks separated by only half a diameter of the smaller one. A mean of two measures of position, rather hastily taken, gave the angle 306° 45′. The central distance must now be extremely small.

As the small star in the neighbourhood of *Procyon* has attracted some attention lately, I send the following measures of it in position and distance, made by Mr. Marth and myself, and would have sent them earlier if they had tended to establish the hypothesis of any physical connexion with the large star:—

1864·15	position	295°·72	Mean of 6 measures, greatest diff. 2°·73				
	distance	44″·95	,,	3	,,	,,	0″·29
1864·20	position	296°·97	,,	6	,,	,,	2°·00
	distance	44″·30	,,	6	,,	,,	0″·94
	position (L.)	295°·19	,,	5	,,	,,	1°·47

I come now to the chief object of my letter, which is to hand you the results of our observations of *Uranus* and *Neptune*, which, remembering what I was able to do in the splendid, and, as it now appears, exceptional season of 1852, are much less numerous than I expected. It must be remembered, however, that a much finer sky is demanded by a 4-foot than a 2-foot aperture. Except where the letter L is appended, the observations have been made by Mr. Marth, who continues to add to our list of new nebulæ.

I have made a good many drawings of planetary and other nebulæ, but have been so much disappointed in some attempts I made to have them faithfully represented in England, that I hesitate about their publication. I have lately tried an Italian artist, whom I have met with here, and I inclose a specimen of his work, which pleases me somewhat better, though the shading-off of the nebula is somewhat too abrupt, and the proportional magnitudes of the stars are not perfectly preserved.* It is the large nebula, *Messier* 20, R.A. $17^h 54^m \cdot 2$, N.P.D. $113° 2'$ (1865), and is figured in Sir John Herschel's *Cape Observations.*

We are at present much occupied with *Saturn* in observing, amongst other phenomena, conjunctions of the satellites (especially the closer ones) with the poles of *Saturn*, or with tangents to the limbs or ansæ of the ring, with a view to contribute towards a more perfect knowledge of the forms of their orbits.

Malta, May 28, 1864.

Observations of the Satellite of Neptune, and of the Satellites of Uranus. By W. Lassell, Esq.

The Satellite of Neptune.

1863.		G.M.T. h m			Measures.
Sept.	10	12 0·9	pos.	215°·69	5
		12 39·3	dist.	14″·15	6
		13 6·4	pos.	213°·96	5
	11	11 48·0		212·15	10
		12 35·4	dist.	15″·95	2
	14	12 42·0	pos.	30°·34	4
	17	11 34·5		210·55	6
	19	11 37·1		33·50	6
		12 35·6	dist.	15″·52	3
	23	12 27·0	pos.	211°·69	8
	28	11 18·2		215·12	6
Oct.	5	10 6·8		212·68	4 L.
		10 8·9		211·73	4
	6	11 1·0		39·3	2
	10	10 1·0		213·80	8
	31	8 30·0		34·11	8
		9 14·6	dist.	16″·25	8
Nov.	14	8 11·6	pos.	216°·51	6
Dec.	11	6 41·9		35·14	8
		7 23·9	dist.	16″·92	8

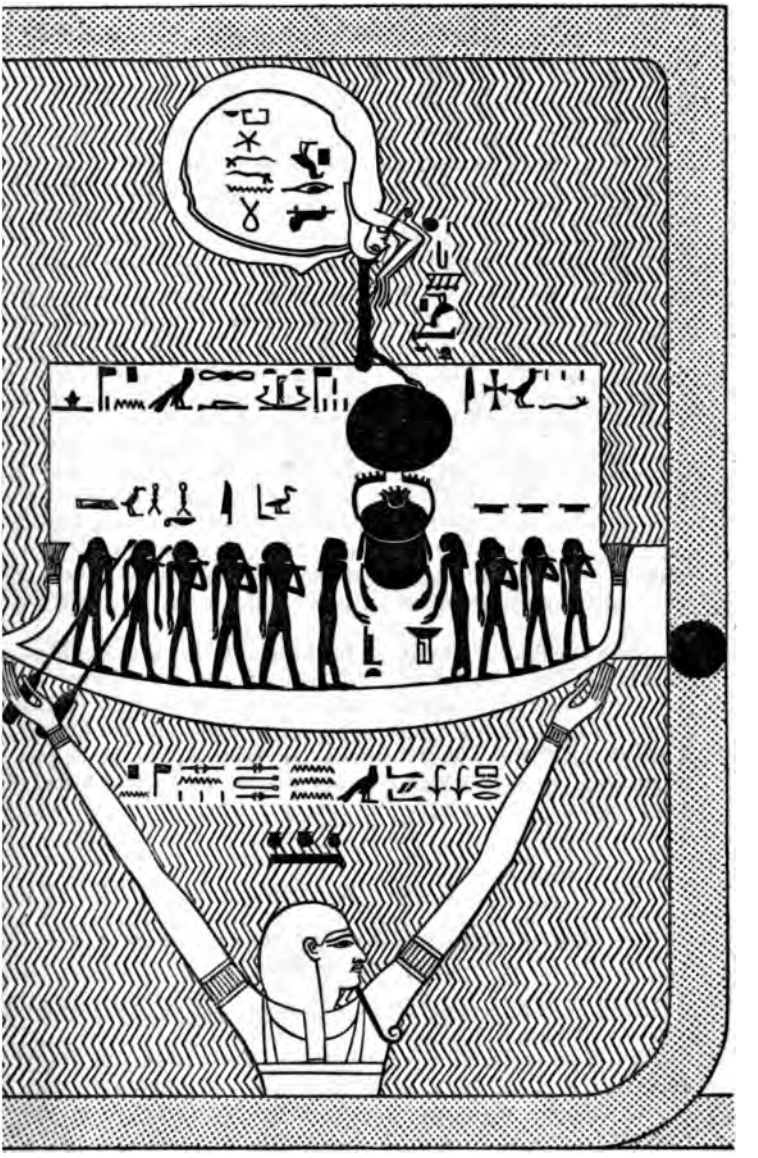
* An engraving from the drawing referred to will be given in the volume of the *Memoirs.*—Ed.

Observations of the Satellites of Uranus.

	Oberon.					*Titania.*			
	G.M.T.				Obs.	G.M.T.			Obs.
1863.	h m					h m			
Dec. 14	10 38·5	pos.	41°·14		2	10 33·4	pos.	71°·11	2
	11 7·9	dist.	45″·27		2	11 0·0	dist.	33″·76	,,
(L.)	11 17·6		44·85		3				
16	10 37·2	pos.	347°·46		2	10 38·1	pos.	345°·64	,,
1864.									
Jan. 22	7 44·3		81·88		2	7 53·1		262·65	,,
	8 13·0		82·24		,,	8 4·7		261·80	,,
27	8 5·6		307·44		,,	8 4·5		53·18	,,
28	7 43·3		281·05		,,	7 35·4		12·40	,,
	7 49·9		282·14		,,	7 57·2		12·15	,,
29	9 42·8		254·20		,,	9 50·1		327·93	,,
	10 12·4		252·61		4	10 1·8		329·35	,,
Feb. 5	6 49·9		66·81		2	6 43·8		45·25	,,
6	6 28·7		41·20		4	6 29·3		3·42	4
26	8 17·8		226·79		2	8 11·7		255·71	,,
	8 53·4		225·44		,,	9 0·5		252·85	,,
27	8 16·5		196·29		,,	8 11·5		210·41	,,
	8 50·1		197·31		,,	8 57·1		209·84	,,
Mar. 2	7 26·6		93·04		3	7 36·0		49·51	,,
3	8 17·5		65·08		2	8 25·3		3·33	,,
7	7 15·8		318·83		,,	7 44·0		200·89	,,
	8 27·8		317·74		,,	7 50·0		200·80	,,
8	8 26·2		291·38		,,	8 34·4		157·29	2
10	6 46·1		240·21		,,	6 53·1		77·25	,,
(L.)	7 32·4		239·41		3	7 24·3	(L.)	77·26	2
(L.)	7 50·0	dist.	43″·14		1				
	7 56·8		43·03		3	8 9·6	dist.	33″·09	3

	Umbriel.					*Ariel.*			
	G.M.T.				Obs.	G.M.T.			Obs.
1863.	h m					h m			
Dec. 14	10 48·8	pos.	8°·99		2	10 42·3	pos.	86°·79	2
1864.									
Feb. 26	8 34·3		72·42		,,				
	8 44·8		72·19		,,				
27	8 2·9		344·71		,,	8 26·1		185·76	,,
Mar. 2	7 53·3		0·86		,,	8 6·6		333·30	,,
3	8 33·1		269·61		,,				
7	7 35·7		289·30		,,	7 25·4		346·78	,,
	8 4·2		287·80		,,	8 15·6		343·15	,,
10	7 6·0		27·14		,,	6 59·6		281·74	,,

On the Astronomy of the Ancient Egyptians, as shown on the Belzoni Sarcophagus. By Mr. Bonomi.



It may not be uninteresting to Astronomers of the present day to understand from an authentic document what were the

notions entertained by the most learned people of antiquity, 1250 years before our era, respecting the construction of the Universe.

The drawing is faithfully copied from a diagram engraved on the wall of a sarcophagus, which was made to contain the body of a king of Egypt, of the period above mentioned, viz., 1250 years before our era.

In the upper part of the diagram is a figure of Osiris, curled up in the attitude of a tumbler, to represent the Earth floating on the surface of the great ocean, which is here signified by the zigzag lines that surround him. This idea of the Earth floating on a vast expanse of water was entertained by all the nations of antiquity, and there is allusion to it in the 24th Psalm, wherein it is stated that the Almighty "founded it upon the seas, and established it upon the floods:" and again, in Psalm 136, verse 6, "To him that stretched out the earth above the waters." Likewise, in the Apocryphal writings, as, for instance, in the 2d book of Esdras, chapter 16, verse 58, it is said, "He hangeth the earth upon the waters."

So likewise in the first century of our era, in the second epistle of Peter, it is said, "By the word of God the heavens were of old, and the earth standing out of the water and in the water;" showing that, although the globular figure of the Earth might have been satisfactorily demonstrated by Demosthenes of Cyrene to a few learned men in Alexandria 200 years before, it was still the popular notion at the time Peter wrote his second epistle.

The little figure standing on the head of Osiris is the goddess of heaven, Netpè, supporting the Sun, which is connected by the Scarabæus with certain divinities in a boat, held in the outstretched arms of a figure, supposed to represent the Ocean. This part of the picture relates to some Egyptian dogma respecting the soul, which has not yet received a satisfactory explanation.

List of Radiant Points of Shooting-Stars.
By Professor Heis.

(*Extract of a Letter from Alexander S. Herschel to Mr. Pritchard.*)

Mr. Williams having kindly sent to the Astronomer Royal at Münster (Dr. Heis), at my request, the bundle of star-charts filled-in with shooting-stars by Mr. Greg, I to-day received a reply from that gentleman, containing a list of Radiant Points observed in eleven years before 1860, at other times of the year than in August and November. Dr. Heis purposes to publish the account of these observations at length

in the ensuing winter. The list I now enclose, with the remark that δ *Virginis* is there pointed out as a perspective vanishing point of shooting-stars, observable *in the months of April and May.* This position I detected by independent observation, *in a single evening*, on the 10th of April last. On the morning of the 20th, following, meteors were also watched for, and were again plentiful. The perspective vanishing point of their courses was fixed with all the accuracy desirable at *Wega Lyræ.*

α *Lyræ*, like the foregoing star δ *Virginis*, is an acknowledged Radiant Point, having been observed by Herrick, in America, on the 19th April, 1839 (*American Journal of Science*, 1st series, vol. xxxvi. p. 362). The observations on the 10th and 20th April, 1864, establish, therefore, two important points:—

1. The interval of a quarter of a century may produce *no sensible alteration in the direction of a meteoric stream.*

2. Two meteoric streams may arrive at the Earth at an interval of not more than ten days apart, *almost exactly transverse to one another in direction.*

These facts are exceedingly difficult to explain upon general principles: both are, however, perfectly conformable to the hypothesis that shooting-stars are cosmical bodies revolving round the Sun.

Radiant Points of Shooting-Stars, Jan.—Dec.

			α = R.A.	δ = N. Decl.
Jan.	1—15	K_1	242°	+51°
		A_1	29	+50
		M_1	135	+57
		N_1	285	+84
Jan.	16—31	K_2	235	+62
		A_2	15	+63
		M_2	166	+52
		N_2	N. Pole	
Feb.	1—14	A_3	65	+51
		M_3	150	+60
		N_3	N. Pole	
Feb.	15—28	A_4	91	+37
		M_4	130	+63
		N_4	250	+83
		S_1	170	+11

		α = R.A.	δ = N. Decl.
		°	°
March 1—15	A_5	54	+47
	M_5	125	+52
	N_5	340	+80
	S_2	178	+ 7
March 16—31	A_6	35	+45
	M_6	140	+50
	S_3	173	+23
	N_6	N. Pole	
April 1—15	A_7	80	+45
	M_7	160	+53
	S_4	181	+18
	N_7	260	+86
April 16—30	A_8	10	+54
	M_8	150	+61
	S_5*	194	+ 5
	N_8	265	+83
May 1—31	S_6*	198	+ 4
	N_9	290	+80
	Q_1	218	+20
	B_1	332	+60
June 1—30	B_2	333	+42
	Q_2	242	+12
	N_{10}	750	+83
	W	292	+15
July 1—15	A_9	41	+62
	B_3	315	+54
	Q_3	262	+12
	N_{11}	20	+85
Aug. 15—31	A_{10}	35	+61
	B_4	306	+59
	N_{12}	295	+79
	T_1	314	+15
Sept. 1—15	A_{11}	35	+63
	B_5	293	+57
	N_{13}	130	+84
	T_2	343	+10
	R_1	53	+35

		α = R.A. °	δ = N. Decl. °
Sept. 16—30	A_{12}	44	+63
	B_6	311	+65
	N_{14}	65	+84
	T_3	1	+11
	R_2	46	+37
Oct. 1—15	A_{14}	51	+61
	B_7	315	+65
	N_{15}	105	+88
	R_3	45	+32
Nov. 19—30	A, N, C, L	Agreeing nearly with the points determined for Nov. 10—13.	
Dec. 15—31	A_{16}	37	+59
	N	340	+34
	M_9	137	+59
	K_3	235	+52

Collingwood, Hawkhurst,
May 10, 1864.

* The Right Ascension and Declination of δ *Virginis* are the following, viz., $\alpha = 192°$, $\delta = +4°$.

On a Zenith Sector for the use of the Great Trigonometrical Survey of India. By Lieut.-Col. A. Strange.

In former times the Zenith Sector employed on the British Ordnance Survey and on the Great Indian Survey consisted essentially of a long telescope, suspended by its upper or object-glass end, and traversing over a short arc at its lower or eye-end, the radius of the arc being equal to the focal length of the telescope. In all cases reference was made to gravity by means of the plumb-line. This form of instrument involved some uncertainty as to the centre of the circle of which the short arc or sector formed a part; and the flexure of the telescope and the effects of expansion on the angular value of the arc were anxious considerations; moreover, the plumb-line is an adjunct difficult to use, and inferior in point of delicacy to the spirit-level; further, observations to the same star could not be taken in reversed positions of the instrument on the same night: and, finally, the instrument required for its

support a lofty and cumbrous fabric of wood, very inconvenient to move and set up.

This form of Sector by Graham and Ramsden is well represented in Pearson's *Practical Astronomy*. That by Ramsden is remarkable for the ingenuity with which it abounds. This fine instrument was unfortunately destroyed by fire in the Tower of London.

A new instrument was therefore designed for the Ordnance Survey by the present Astronomer Royal. It is fully described in the Ordnance Survey Reports, from which I extract the following passage:—

"The first principle in this Instrument, now known as Airy's Zenith Sector, was the arrangement for making successive observations in two positions of the instrument—face East and West at the same transit. The second principle was the substitution of a level or system of levels for the usual plumb-line. The third principle was the casting in one piece, as far as practicable, of each of the different parts of the instrument, in order to avoid the great number of screws and fastenings with which most instruments are hampered, and to secure, if possible, perfect rigidity."

As to the soundness and the progressive character of these principles, no competent judges can differ. The instrument itself is one of the most original ever devised, and the work executed with it is of a very high order.

When entrusted by Government with the superintendence of two Zenith Sectors for the Indian Survey, it might have sufficed that I should simply adopt the plan of the Astronomer Royal; but this was prevented by a condition imposed upon me to which he had not been subject, namely, that of extreme portability combined with equal power.

Mr. Airy's instrument weighs altogether 1140 lbs.—a bulk of metal which constituted it, in the words of Colonel Walker, the head of the Indian Survey, "an instrument evidently unsuited for crossing the higher Himalayan ranges."

An examination of the drawings of the instrument, to which I must refer the reader, will show that its weight is chiefly due to the manner in which the azimuthal motion is obtained, namely, round two points external to the telescope and sector. The abandonment of this primary feature involved the abandonment of the whole design; and the subject had to be taken up *de novo*—Mr. Airy's fundamental principles, above cited, being however preserved as a basis.

I hope on some future occasion to furnish a detailed description of the form at which I have arrived, and shall now merely indicate its general characteristics.

Place a German Equatoreal as it would be at the Pole—that is, with its polar axis vertical. Attach to the telescope, but at right angles to it, a frame carrying at either extremity about 40° of a circle. Attach to the declination-socket four

microscopes opposite the above-mentioned arcs. Then the polar axis of the Equatoreal becomes the vertical axis of the Zenith Sector, and the declination-axis the horizontal or transit-axis. The telescope being pointed to the zenith, the sector-frame will be horizontal. The microscopes are arranged in the same way as those of the Greenwich Transit-circle, and have the same description of illumination.

In this arrangement there are certain features that appear to me to be advantageous:—

1st. The vertical axis is screened from sudden changes of temperature within a hollow cast-iron column.

2d. The sector does nothing but bear its own weight.

3d. Its two arcs, being at the extremities of a horizontal diameter, are both under the same conditions with respect to flexure and exposure to temperature.

4th. The telescope turns on a horizontal axis, is of considerable length, and rests on invariable bearings. It and the attached sector will therefore describe a true plane—a condition necessary to the constancy of the microscopic runs, and to the performance of the instrument as a transit-instrument.

5th. The Greenwich or axial system of microscopic illumination, here adopted, is the best at present known.

6th. The eye-pieces of the microscopes, being all four nearly at the same elevation, and that a low one, are easily accessible to the observer without climbing.

There remains only the weight of the instrument to be considered. The work, executed by Messrs. Troughton and Simms, is now in a sufficiently advanced state to enable me to make a close approximation to what it will be. The whole will weigh not more than 660 lbs., or very little more than half the weight of the Ordnance Survey instrument. The weight of the heaviest piece will be 110 lbs., as against 254 lbs., the weight of the heaviest piece of the latter instrument. But I should add that, in the instrument under description, the sector is 4 inches less in radius than in Mr. Airy's instrument; the telescope, however, being the same. I do not believe this diminution of the sector will affect the power or accuracy of the instrument. The dimensions in my instrument are,—focal length of telescope, 48 inches; diameter of circle of which sector is part, 36 inches; or ratio of sector to telescope = 1 to 1·33. In the Greenwich Transit-circle the ratio is 1 to 1·95; the new Paris Transit-circle is about the same; Ertel's and Repsold's Pulkova Circles have ratios respectively of 1 to 1·62 and 1 to 1·75. The proportions I have adopted, as compared with these important modern works, give a preponderance to the circular over the optical member of the instrument.

London, 1864, *June* 10.

On Dr. Steinheil's Stellar Spectroscope. By W. G. Lettsom, Esq.

At page 105 of Vol. XXIII. of the *Monthly Notices*, in the abstract by Mr. Drach of Professor Donati's *Memorie Astronomiche*, the author speaks of stellar light as never being strong enough to enable one to distinguish its colours; and he states further that, saving a reddish tinge at the minimum-refraction end, the spectra appear of a lavender tint in their whole length.

This may be the case with Professor Donati's apparatus; but with the stellar spectroscope of Dr. Steinheil, of Munich, the colours of the stellar spectra are as distinctly pronounced as when one examines the light of the Sun by means of a prism.

In the spectra of the different stars there is, it is true, a remarkable difference in the colours shown; yet the variety of hues in by far the greater number of those spectra is admirably exhibited.

I find the spectra of the larger stars to be displayed with such brightness, when using the spectroscope in question with my 9-inch refractor, that I can hardly doubt it would give very satisfactory results with stars up to the second magnitude, when applied to instruments of not more than four inches aperture.

The cost of this apparatus, which may be adapted to any instrument without difficulty, is 66 Bavarian florins, or about 5*l.* 12*s.*

Monte Video, 24th March, 1864.

On the Method of determining Heights in the Trigonometrical Survey of India. By Lieut.-Colonel J. T. Walker, R.E. (Abstract.)*

From the origin of the Survey until the year 1856 the heights of all the principal Stations of the Triangulation were invariably determined by the method of reciprocal vertical angles. But the difficulties which beset observations of vertical angles in the plains of India are so great, and the inaccuracies to which they are liable so serious, that these observations were a cause of much anxiety and uncertainty, because, if erroneous, the lengths of the base-lines and all operations emanating therefrom would be erroneous. It was consequently deter-

* This paper has been ordered to be printed *in extenso* in the volume of *Memoirs*.—Ed.

mined to undertake a series of spirit-levellings to fix the heights of the base-lines, and verify a certain number of principal stations, more particularly those situated in the plains previously referred to. The main lines of levels were executed by at least two persons, each furnished with a standard level and pair of staves, one following the other, station by station, at a few minutes' interval. The various sources of error, and the precautions taken in order as far as possible to remove them, are detailed and discussed. It is stated that a comparison between the results of the spirit-levelling operations and those obtained from the Principal Triangulation is found to be highly satisfactory in all instances where the vertical angles were measured at the time of minimum refraction, even where the triangulations had been carried for long distances over the plains already noticed. The average difference between the results obtained at the ends of four circuits, averaging 550 miles in length, is 3·06 feet, and the maximum difference at any station is 8·7 feet, which occurs at a station in the valley of the Indus, 200 miles from the nearest hill station.

In combining the trigonometrical with the levelled values, the latter are assumed to be correct and final; and stations obtained by the former process alone are referred to the nearest station of the lines of levels. The Survey Department has published a volume of Tables of Heights in Scinde, the Punjab, North-West Provinces, and Central India; and the paper was chiefly compiled from the Preface to that volume.

On the Star β Leonis. By G. Knott, Esq.

In his invaluable *Cycle of Celestial Objects*, Admiral Smyth describes this star in the following terms:—"Position 114°·0 (w. 2); distance 298″·0 (w. 1); epoch 1833·47. A standard Greenwich star with a distant companion, on the switch of *Leo's* tail. A 2½, bluish; B 8, dull red; preceded by a 7th-magnitude star in the *n. p.*" And I am informed by the Rev. W. R. Dawes, who kindly called my attention to this object, that in Admiral Smyth's original diagram three stars are figured, the nearest (the B of the above extract) in the *s. f.* quadrant, and the other two at greater distances in the *n. p.* and *s. p.* quadrants respectively.

The present appearance of the field differs in several respects both from the description and the diagram; and I find that in his "*Second Series of Observations of Double Stars*," printed in the last volume of the Society's *Memoirs*, Mr. Powell of Madras suggests that there is an error of quadrant in the *Cycle*, that the angle of position of B should be increased by 90°,

and that the 7th-magnitude star should be described as in the *s. p.*, instead of in the *n. p.* quadrant. This emendation, though not supported by the *diagram*, would bring the *description* into close accordance with the main features of the field as seen in the telescope; but, if this should prove to be the true solution of the difficulty, I am desirous of calling attention to the curious fact that there happened to be a minute star very near to the place of the B of the *Cycle*, the co-ordinates of which, from a series of wire-micrometer measures with my 7½-inch Equatoreal on four nights are, P=115°·39, D=303″·6, epoch 1864·38. Its magnitude I estimate at about 11·7 of Mr. Pogson's standard scale (corresponding to about the 13½ mag. of Admiral Smyth's scale); and Mr. Baxendell informs me that he had himself observed the star some years ago, and estimated it to be of about the 12th magnitude. For the two stars mentioned by Mr. Powell, and which are Nos. 160 and 162, Hora XI. of Piazzi's Catalogue, I obtained the following results: for the former, which is of the 6·5 mag., P = 201°·6, D = 18′·9 ±; and for the latter, the magnitude of which I estimate at 8·4, P = 206°·37, D = 282″·4. Epoch 1864·40. In comparing these results with those of other observers, it must be remembered that *β Leonis* has a decided Proper Motion.

Woodcroft Observatory, Cuckfield,
June 10th, 1864.

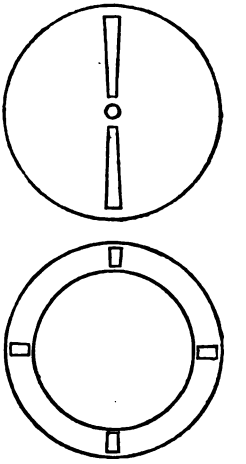
Since writing the above, I have, from the places given in the Palermo Catalogue, deduced the angles of position and distances relative to *β Leonis*, of Piazzi's Nos. 160 and 162, Hora XI., with the following results:—160 P XI., 6·7 mag., P = 202°·5, D = 19′·1; 162 P 11., 8 mag., P = 209°·6, D = 301″·10. Epoch 1800·0.

It may, perhaps, save the risk of confusion if I remark that, in addition to the small star mentioned in the previous communication as agreeing fairly in place with the B of the *Cycle*, there is in the same quadrant a brighter star, 10·6 mag., *n* = 116°·16, D = 556″·5, which has a small comes to the P.*f.*, the co-ordinates of which are P = 21°·8, D = 50″·6.

Woodcroft Observatory, Cuckfield,
June 15th, 1864.

In a letter, dated August 8, addressed to the President, Sir John Herschel proposes an eye-piece for viewing the Sun should be constructed, in which the eye would be defended by

a revolving metal plate, having in it one or two very narrow slits — say two, diametrically opposite, each occupying a sector of the disk — say half a degree. The diaphragm being made to revolve — say five times in a second — a glimpse would be caught at intervals of $\frac{1}{10}$ of a second, which suffices for continuous vision. But the *quantity* of light which would enter the eye would be only $\frac{1}{360}$th part of the total light of the Sun. If the disk were large — say an annulus of 10 inches diameter—and the slits, four in number, were each $\frac{1}{100}$th of an inch in breadth, the annulus revolving once in $\frac{2}{3}$ of 1 second, the quantity of light would be reduced to $\frac{1}{785}$; and if the surface of the annulus next the Sun were of polished metal or looking-glass, the annulus itself would not become very hot.



The annulus and the apparatus setting it in rotation might, of course, be disconnected from the telescope so as not to communicate tremors; for example, fixed to the observer's stage, but, if connected to the tube, a mode of attachment might be devised *cutting off tremors* by the different elasticities of the connecting materials, upon a principle which he devised some years ago, and which has been applied with perfect success (in one of its forms) by the Astronomer Royal to destroy tremors in his mercurial reflector for the Great Circle.

M. Schiaparelli, in a paper entitled "Théorèmes sur le mouvement de plusieurs corps qui s'attirent mutuellement dans l'espace," *Ast. Nach.* No. 1487, establishes, by a simple analysis, the following theorem:—

"When three bodies move in space under the influence of their mutual attractions (according to the inverse square of the distance), the three osculating planes of the trajectories and the plane of the three bodies meet in a point.

"This point is the centre of gravity of the three bodies, each mass being augmented proportionally to the cube of the distance of the other two bodies."

He remarks that the theorem properly modified extends to a system of n bodies attracting each other according to any law; the theorem being, in fact, obtainable from the consideration that, imagining the bodies to become rigidly connected,

the solid body so obtained is in equilibrium under the influence of the mutual attractions. Hence, considering the n resultants of the attractions which act on the n bodies respectively, these results must be forces in equilibrium as regards the aforesaid solid body, and must therefore satisfy the conditions which exist for a system of forces in equilibrium.

In the case of three bodies, the resultants must be in a plane and meet in a point. In the case of four bodies, they must (in accordance with a theorem of Möbius) be generating lines (of the same kind) of a hyperboloid. Whence also the osculating plane of the trajectory of each body cuts the resultants on the three other bodies in three points which lie in a line.

In the case of five bodies, the resultants act along five lines such that there exist two lines cutting all the five lines.

And, in the case of six bodies, the resultants act along six lines connected by the relation which Prof. Sylvester (*Comptes Rendus*, t. lii. p. 741) calls *involution in space*.

The author concludes by remarking that the resultants, whatever be their number, satisfy the six general equations of equilibrium of a solid body. Of these equations, three express that the sum of the components of the forces parallel to three axes is equal to zero. The first three equations lead to the theorem of the conservation of the motion of the centre of gravity; the last three to the theorem of the conservation of areas.

Comet I. 1864.

Discovered by M. Tempel, at Marseilles, July 4.

The following set of elements, calculated from observations of July 9 and 21, and August 5, by Mr. Graham, of the Cambridge Observatory, are given, *Ast. Nach.* No. 1490:—

$$\begin{aligned} T &= 1864, \text{ Aug. } 15{\cdot}533685 \text{ Greenwich M.T.} \\ \pi &= 246^\circ\ 14'\ 32''{\cdot}9 \\ \Omega &= 95\ \ 14\ \ 45{\cdot}2 \quad \text{Mean Equinox, Jan. 16.} \\ i &= 178\ \ 7\ \ 53{\cdot}5 \\ \text{Log } a &= 9{\cdot}958759 \end{aligned}$$

The middle place is represented thus:—

$$C - O, \text{ in Long.} = +\,0''{\cdot}4, \text{ in Lat.} = -\,4''{\cdot}8.$$

Comet II. 1864.

Discovered by Prof. Donati and M. Toussaint, at Florence, July 23.

The following set of elements, calculated from observations of July 28, July 31, and August 2, are given by M. Krüger, *Ast. Nach.* No. 1487:—

$$T = 1864, \text{ Oct. } 11{\cdot}088 \text{ Greenwich M.T.}$$

$$\left.\begin{array}{rcl} \varpi & = & 159^\circ\ 0'\ 26'' \\ \Omega & = & 31\ 59\ 12 \\ i & = & 70\ 23\ 0 \end{array}\right\} \text{Equinox, July 31}$$

$$\text{Log } q = 9{\cdot}96400. \quad \text{Motion Retrograde.}$$

Middle Observation, C – O, in Long. = – 4″, in Lat. = – 3″.

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